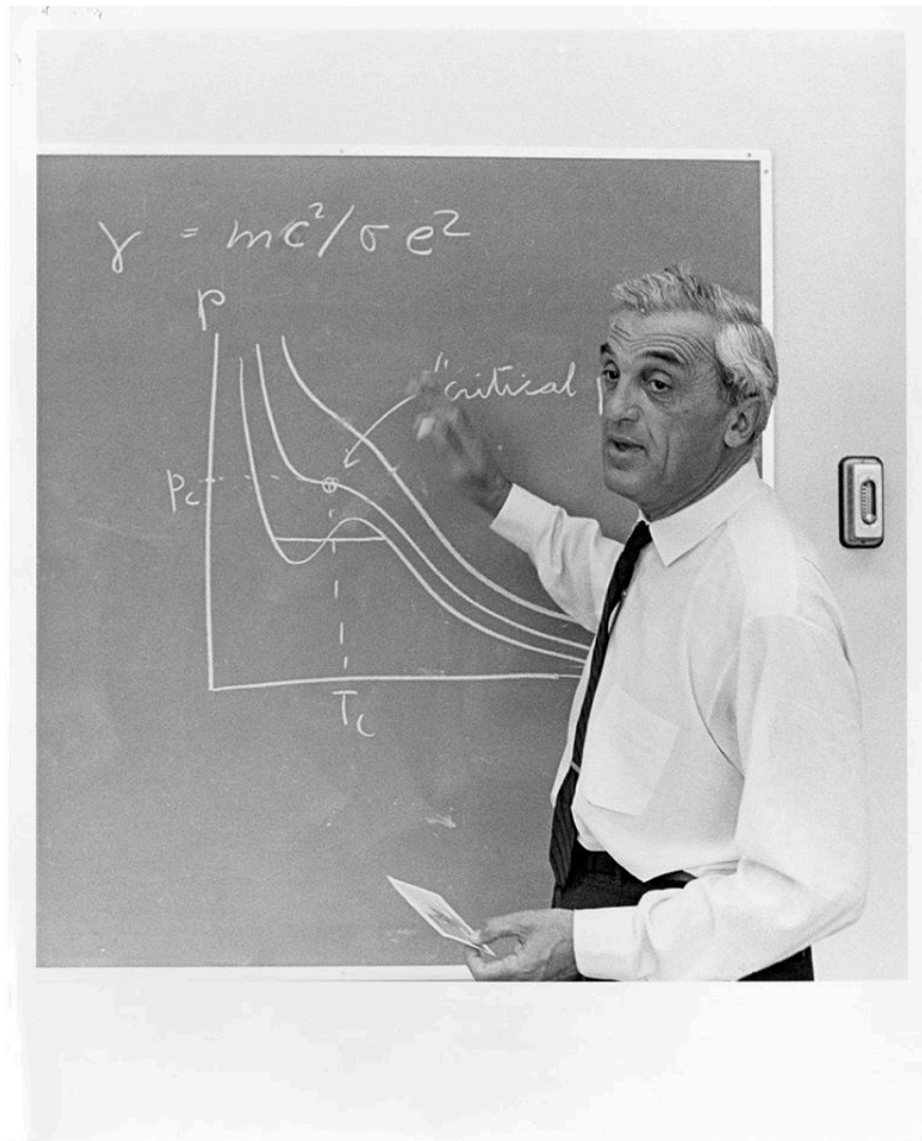


Interviews with Prof. Felix Bloch



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Pure Mathematical Physics

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Interview Session – 1

Bloch:

Well, of course, I do not know. I mean if you are only interested in the history of quantum mechanics, of course by the 1930's it's essentially established, in fact has been established before. However, let's see.

Kuhn:

...Our policy as to where we cut off has simply been: Let us go to the point where the principles are clearly established and the interpretation is generally accepted, and then with each of the major applications, let us go to the point where it becomes apparent to people that this is going to work out. I mean the principles — that now it is a matter of doing the details.

Bloch:

That, of course, was really very early.

Kuhn:

Remarkably, except in a few fields, field theory, for example; there we've tried to explore a little further with that. I'd like to know more than we do know about that transition from, in the early '30's, quantum mechanics to nuclear physics, and the application of quantum mechanical principles to the nucleus. I know, for example, and it was probably happening already when you were at Leipzig, that Heisenberg was worrying about the problem of the electron in the nucleus —

Bloch:

Oh, I see. Oh, well, he had the answer there. I mean he took me on a walk sometimes and told this.

Kuhn:

This sort of thing I do want. But...I'd be interested first in finding out something about your own biographical background. I'm interested in the new group who came into physics in these years. I'm also interested in Switzerland, because although you're not the only person who went to school in Switzerland, you're one of the very few people who

was born and brought up and became a scientist in Switzerland.

Bloch:

So you want me to tell you briefly about my family? Both my parents were born in what was at that time the Austrian Monarchy. My father was born in what is now Czechoslovakia, Bohemia, the German-speaking part, and my mother was born in Vienna. For whatever interest this has, they were actually first cousins, which happened rather frequently in my family. My father came to Switzerland in the late 1890's and settled there, and my mother then came over later. I had a sister first, but she died young. She was born in 1902 and died in 1914, at twelve years. I was born in 1905, and my father became a citizen of Switzerland about that time, and I think actually — I don't even know whether I was already a citizen when I was born, but if not, I became one through my father a year or two later. My father was a businessman, not terribly successful himself, rather more interested in educational things. I remember my mother said, and quite rightly often, that my father really would have liked best to be a teacher. He liked to read books; he was not particularly

interested in sciences, but he was fascinated by numbers and so on. His real interests were in geography, languages — he was very good in that.

Kuhn:

What had brought him to Switzerland in the first place?

Bloch:

Oh, he had an uncle; his uncle was already (situated) and was in the wholesale grain business. His uncle had moved over there and evidently took the promising young nephew into his business. Then my father decided to stay there, that's all. It's strange because the name "Bloch" is very common in Switzerland, but most of them come from the Alsace. I still have a cousin of my father who lives in Zurich, and he and his daughter — I think they are the only Blochs that come from that part of the world. Well, I mean of course we were a Jewish family, but not of the religious kind at all. My father was not interested in religion. I actually was to some extent in my early days and still am, maybe partly as a reaction to my father's atheism. That you won't really print. I mean you'll use these words

judiciously. Most of these you're not going to print anyhow...? When it comes to choice of profession, I don't really quite recall how far it goes back, but certain interests in mechanical things, at least, I must have had rather early because — I don't remember how far back it goes, but I said that I was wanting to become an engineer. Now, in my family, they were mostly in business. My mother's brother, my uncle from my mother's side, owned a small textile factory in Vienna; so he was in that sense a bit of a technician, but also a businessman. There were some doctors in the family, but no scientists at all.

Because of this inclination — I really don't quite recall how it came — about the nearest practical thing was engineering. And it was very appealing to me; I liked bridges and beautiful things like that. Like everybody probably, I was interested in steam engines and locomotives at that time. And so, during the whole time of my Gymnasium, high school, and I don't know — even perhaps earlier — I think whenever I was asked what I wanted to be, I said I was going to be an engineer. This was fine with my parents and was strongly encouraged by this uncle of mine who felt that this was really what he was missing. He thought it was a good idea nowadays to

have a more scientific background, instead of purely empirical, which he had, although he was a very capable man.

Kuhn:

This is the uncle who was in the wholesale grain business?

Bloch:

No, no, who had the factory. I mean he had to deal with chemical processes, mechanical processes and so forth. Well, he thought maybe sometime I might go into the same business, but should be better prepared academically than he was. I think he went only through high school, had no university training. I did then go in the Gymnasium in Switzerland. The schools were very good in Switzerland. The elementary schools were easy, and we didn't learn too much, but they were very humanely run. We had a lot of time; I did a lot of reading and so on. I may perhaps mention one thing which I think is of some interest, and that is, the death of my sister, to whom I was very much attached, made me into an extremely lonely child. After my sister died, I was really not very much interested in the company of

other children, because the grief which I had I felt I couldn't share. And consequently — I was at that time only nine years old —. Then I was the only child. I was a doubly only child, because my sister died very suddenly, from blood poisoning, in a very short time; it was a great shock to my parents. Then, of course, they became doubly attached to me, attached and also doubly careful. I was not allowed to do many things that other boys were allowed to do because they were afraid that something might happen to me too, so I became quite a secluded person at that time, I think against my real natural temperament. And so I did a lot of reading and became a very good student; I can say that without exaggeration. In the Gymnasium, although I was good in mathematics, I couldn't say this was one of my prime interests. I liked Latin very much; I was very much interested in languages and found them very easy. I studied languages, took some music at that time like every boy of this class — played my piano, didn't like it too much, but some. Then I became also very much interested in Nature; the Swiss mountains meant a great deal to me. And, actually I just felt, 'Well, all right, engineering seems a reasonable profession.' I liked mathematics too,

like other things. My very first interest which gave me some pride — I think in our school library, probably when I was in the first or second class of the Gymnasium, I would say maybe twelve or thirteen years old — I got an old book on astronomy, I think Newcomb, I think it's called. I thumbed through that, and although I didn't yet know much trigonometry, I was very much fascinated; and just for myself I solved some problems like, 'How does the length of the day vary between spring and fall?' I (developed) an approximate formula and checked that it agreed, and it gave me a great deal of satisfaction. So I began sort of to feel, 'well, maybe I am really capable of doing this thing,' but still only with the idea, 'Well, apparently I will not have any difficulty.' Many boys were afraid in my generation: 'Oh, engineering, that takes so much mathematics.' That didn't scare me at all.

Kuhn:

The resistance to science and engineering was often expressed as fear of mathematics?

Bloch:

Yes, quite right.

Kuhn:

That's very prevalent today, but I have never been altogether sure where it came from or how old it was.

Bloch:

Since I wanted to go to the Technische Hochschule which was the training for engineering, I decided I would go to the Realgymnasium, which was less humanistic and which essentially meant that I didn't study any Greek, but I did have Latin. But even in that class there was a certain — well, the humanities were the thing. My fellow pupils at that time thought that mathematics was something rather sober and dull and so forth, and also difficult. Since I didn't find it difficult, it gave me some pride, and they came to me and I helped them in solving their problems and this sort of thing.'

Kuhn:

You spoke earlier of having done a lot of reading as a child and particularly in this period of great isolation after your sister's death. What sorts of things did you then read?

Bloch:

Oh, all kinds of things, whatever entered my hands — mostly literature — stories, travel stories, this kind of thing. The first thing that I remember, although I probably thumbed through some books like it, is this astronomy book which fascinated me very much. Then my father took me once to an observatory at night and I looked up and saw some stars; I had the feeling that this was very wonderful and apparently people can even understand what goes on up there, although they didn't understand so much at that time. I had quite an early feeling that, 'Yes, Nature is apparently capable of rational analysis'; and also because of the great shock of the death of my sister I very soon got attracted to that because I felt, 'Well, life is uncertain, people die; here is something which is certain; this is a sound foundation.' And I think that, to some extent, was at that time a narrow but firm basis which made me very fond of mathematics. So I was very good in mathematics. Our training was excellent, I must say, at the realgymnasium. Our mathematics professor, who taught us mathematics in the last year, was a man by the name of (Karl) Beck... I remember — that is from your point of view of interest—two

teachers. One is (Karl) Beck, who worked with Pierre Weiss at that time. He was actually, although he taught mathematics, a physicist. And he re-did the Einstein-de Haas experiment — I don't know whether you remember, de Haas got the wrong answer; he got the one that Einstein predicted, which nowadays we would say corresponds to spin one, $e/2mc$. Beck, under the guidance of Pierre Weiss of course, was a very good man in mathematics and found, to his great surprise, that the factor was twice as big, but he had no explanation for that. But I think the (connoisseurs) know that; that the first time this paper — (for de Haas did it and probably) verified it. But it is true that the anomalous effect — the suggestion came from Einstein; one could find out. De Haas did an experiment but did it poorly and found what one would expect from the point of view of classical mechanics; this man did it right. And he was an excellent teacher, so it was really a joy to take mathematics with him.

Kuhn:

Did he talk at all about this work of his own or about his work with Pierre Weiss?

Bloch:

No, no, not at all. This is something I learned later on. No, he taught us what has to be taught —

Kuhn:

The Weiss magneton did not enter into your Gymnasium curriculum?

Bloch:

No, no, not at all. I did not know as much; in fact, I knew he was a good teacher, and I rather liked him, but I didn't know how outstanding he was.

Kuhn:

Now, how far did you go with mathematics in the Gymnasium?

Bloch:

We did not have calculus. Analytic geometry was the last thing we learned, and we learned that very thoroughly, in the old-fashioned way with conic sections and this sort of thing. "At that time —

although I was not premature — when I was almost eighteen", I did get hold of a book on calculus and got the elements, at least, of derivatives. I realized at that time that this was a very simple way of constructing tangents to curves in analytic geometry. That we weren't taught, but this I learned on my own. But that's all. We had also a very excellent teacher in physics; his name was Seiler. S-e-i-l-e-r. He wrote a book on that. Oh, there are many in Zurich who remember him. He was really very enthusiastic about his subject. I do not think that he was scientifically particularly outstanding, but he was a marvelous teacher and taught us the fundamentals very well.

Kuhn:

Now, what did that consist of, the fundamentals?

Bloch:

Mechanics, elementary mechanics, but without calculus. Mechanics, optics, a little bit of heat, some electricity but only DC currents — no electromagnetic theory at all. And we had a laboratory; we had a laboratory in which to determine g , a little school laboratory. I think we

had, except for the calculus, almost as good a training in basic physics as we teach here now in introductory courses.

Kuhn:

Were you given any sense or did you have already in the Gymnasium any sense of the crisis existing in physics, or just classical physics —?

Bloch:

Oh, well, at that time of course Einstein's theory became sort of popular, and many popular books were written about it; I read them and didn't understand them at all. I had only the vaguest idea; I was always told this was something very profound and world-shaking, and I sort of accepted it on good faith and it didn't make too much sense to me. I just didn't get the right book into my hands. Those popular books were rather badly written.

Kuhn:

But the quantum you knew nothing about?

Bloch:

The quantum I don't believe I knew much about. I don't remember where I first met the quantum. No, I really don't think in the Gymnasium. I was still of the opinion that I ought to become an engineer. I must say that towards the end, shortly before the (Matura) — you know, that's when you finish the Gymnasium — I began to have some hesitancy, because at that time my uncle came; I visited him often and he talked to me and he said, "Yes, that's all very fine. I'm glad to hear you're interested, but of course you must also be very sure that you get a sound commercial training to be a good businessman." At that time I began to fear that engineering might not be quite what I wanted because my family was interested in engineering from an entirely practical point of view, and this was not what attracted me very much. Nevertheless, I did enter the Technische Hochschule... I finished high school, so to say, with flying flags; I think I got the best grades. I'm sorry to say that I was not one of these one-sided geniuses, not at all. [Laughter]. I don't know whether (???) I just liked the place; it was a good school; I got along with all of my friends. So I did then enter the Technische Hochschule. I was quite interested in literature too.

In fact I remember that the professor who taught us German literature was sort of (disappointed); he thought I might go into Germanistics. But this never appealed to me at all. I entered the Technische Hochschule in 1924 as an engineer and studied engineering for a year.

Kuhn:

You went to the Technische Hochschule because you wanted to be an engineer, but had you already known that you were going to be a physicist, would you also have gone to the ETH, or would you have gone to the University?

Bloch:

I've never asked myself that question. It's quite likely that I might have come to the ETH anyhow.

Kuhn:

You see, I'm curious about the relation of those two institutions. In physics the ETH has always had, or it usually had, a better reputation.

Bloch:

Yes, it did.

Kuhn:

But that's an unusual — I mean, in Germany, you would very rarely find a major university and a neighboring technische Hochschule where the technische Hochschule was better in physics.

Bloch:

Well, of course, the University went sort of up and down. Of course, Schrodinger was at the University but was not so well known then, at that time. But I think, before that, Einstein also taught temporarily at the University. So the University (branch) went through its ups and downs. At that time, with the exception of Schrodinger who was not very well known, it was not terribly good. There was a man by the name of E. Meyer, and Bar, who was actually quite a good physicist. So the better people were at the — there was Debye who was at the Technische Hochschule; I think I might have —

Kuhn:

He had, however...first come to Zurich to the University, before he was at Gottingen.

Bloch:

Right. And then he came back to the Technische Hochschule. So it's quite possible I would have come to the Technische Hochschule. Anyhow that choice wasn't out to me so(specifically) because I did start as an engineer. Now, fortunately the training of engineers at the Technische Hochschule, at least in the first year, was not very different from that of physicists and mathematicians. Physics and mathematics had a group together — a little school so to say, an Abteilung — for physics and mathematics. But I was in the engineering one. But except for some things like drawing of machines and so on, we had the normal things: calculus, which was not very well taught — I did not have a good professor there — calculus, projective geometry, the usual sort of things. I'm not sure whether I studied already mechanics at that time, whether I started in the beginning — anyway, the training was not very different. But by that time, then, I became pretty sure

that engineering was not my cup of tea. But to make quite sure, in the summer of 1925 — it was recommended to engineering students that they ought to use their summer vacations to do practical work — I did do practical work. I worked as a volunteer in an iron foundry, somewhere near Zurich; and that just about finished it. I mean it was very clear to me that this was not at all what I was looking for. It was a very empirical — it was a small thing, of course; it had no scientific interest. It was a system, to my mind — I was quite leftist at that time — a system of exploitation of the workers, who were very hard-driven and so on. I wanted to have no part of it. And after that I came and I decided at that time — well, I decided, I felt very strongly that this was not it. For a moment I thought of medicine, which my mother encouraged, but I did not mean that too seriously. Then I started to go around and ask people what they thought of my studying physics. I went to my old Professor Seiler and he said, "Don't do it. It's a hard job; there is no meat in it. Look at me here; I'm teaching the same thing since years." I went to Hermann Weyl, who was the director of the Abteil for physics; he didn't know me at all. I reminded him later of it; he couldn't remember it, but there was just

this matter, "Should I study physics?" He said, "No. You shouldn't." He was quite right. There was absolutely no prospect. If a man was that unsure that he had to ask, I think the sensible advice to him was to say "no." Well, I did it anyhow. There were a few people — oh yes, this I might say. This was important. My father's cousin in Zurich is a lawyer. His friend from school days was a physicist by the name of (Jaffe), who was quite good; he was in Giessen. Have you heard his name?

Kuhn:

Yes.

Bloch:

He was a man who worked in the early days of relativity. He's a very old gentleman now; he's still alive. Now, my parents started to ask around and say, "What is this this boy of ours wants to go into, this futureless occupation of being a physicist?" And my uncle — I call him my uncle; he's my father's cousin — asked this man (Jaffe) and he had seen me or something, and had the good sense to tell my parents that they should be very glad that they had a son who knew what he wanted. And so I got a little

bit of encouragement there. My father was rather sad about it, and for years didn't quite see why I chose such an abstract thing. But he was a very kind man and said, "Well, all right, if that's what you want."

Kuhn:

Do you think it was something of a dislike for the field because of its abstraction, or was it the notion of a university career, or that the teaching career was not itself secure enough?

Bloch:

Entirely, yes. Entirely; that my father felt. Well, after all, he was not a very wealthy man; he thought I ought to make a living. The only future that I could see — and that even was uncertain — was to become a high-school teacher. And actually I did at the University take courses in education, because I had no qualifications for going into high school, and because it was more likely than not that that was what I was going to do... It was not easy to get these jobs. They were relatively well-paid and respected in Switzerland. I actually remember when I told it to my father, he asked me, "What do you want to do later?" and I had to say, "No, honestly I don't know."

And that worried him, of course. Well, you must have heard that uniformly from this generation: the prospects were slight unless a man came from a family with scientific background.

Kuhn:

I don't know that anybody has said before that there was real uncertainty even as to the prospects of getting a good high-school teaching job — high school in the American sense. Certainly this problem that nobody could feel any assurance of a university job was uniformly clear.

Bloch:

Yes, (the jobs were filled) very rapidly in the high schools. Jobs were not too abundant, and from what I've told you, it was people with a very high-quality education who got those jobs. It was not trivial. One really had to show that one knew something, one was proficient in teaching, and so forth. So it was entirely a shot in the dark, and I really have thought, rather proudly now, that I did that simply at that time because I thought I couldn't help it; and never mind what it leads to.

Kuhn:

Had you gotten further ideas from your first year at the ETH as to what physics was like, or was this largely based still on what had gone on in the Gymnasium?

Bloch:

No, no, of course. That's right. In the first semesters I began to realize that this was a highly active and interesting field. Although I did not know the details of the problems, I think I did know already that atoms were of interest.

Kuhn:

There do you suppose you got those ideas? What courses might you have been taking, or what lectures might you have been going to?

Bloch:

This I frankly don't recall, because physics — then, of course, I took the elementary physics from Debye.

There it was made very clear to us, but I believe I had already at that time decided to be a physicist. I think I took physics only my third semester. Well, of course, I think that probably went around. Students talked to each other and knew some physicists, and I knew some. I cannot say that with assurance. All I know is that when I got into the first contact with real mathematics and real physics, I realized this is a serious and exciting business. Although I was also worried about the material prospects, that I would feel at home in this field I had very little doubt. I may also say that I had also a little bit of success already, because our teacher in projective geometry was a man by the name of [Marcel] Grossmann, who was an early friend of Einstein. And I think, in fact, he pointed out to Einstein for the first time when Einstein struggled with general relativity, that there was this so-called Ricci calculus. I think Einstein was always grateful for him. Now he was partly paralyzed and therefore not a very good teacher, but his course in projective geometry I found very interesting. We had problem sessions. Those themselves were good people; those were not students; those were people who had finished. I remember a proof in projective geometry, which of

course one gave, which I thought was very involved; yet the result was simple. I thought it over very hard over a weekend, and then sort of timidly, in the problem session, pointed out to this man that one could prove it more simply. And he was a good man; he encouraged me very much and said to his students, "See, one of your fellows here has better sense." So I got encouragement and I felt, "I can do something there." But I was still worried, because I did not know how much I could do, still. All right, so then in 1925 I entered the Abteilung für Physik und Mathematik; in the beginning still not very much happened except this very wonderful course of Debye, the introductory course in physics. Debye had this real fine knack not only of making the basic things very clear to you but indicating, at the right moment, how they are connected with the more modern things.

Kuhn:

Was that simply a one-year series of lectures, or was it one of the standard German four-semester courses —?

Bloch:

One year. It was two semesters. It was two semesters. But every day — I don know, four times a week or so — with laboratory. And I believe even with problem sessions, I don't quite know, The laboratory I often neglected, I remember, and at the end I only learned what I should have known before, that a certain number of experiments was required; and I'd only done half of them. At that time I became a little —. You see, one thing was nice: because of the small material prospects, there were only a few people in this Abteilung. And they formed a sort of brotherhood, even our assistants. Consequently I was already a part of the metier. So this assistant was very nice, but Debye was rather angry that we hadn't done it, and he made us do all the experiments. So on all sides we made six experiments in one afternoon and fulfilled the requirement that way. So I was not terribly much interested in exreriments at that time. Then the first real support — I mean Debye was a man very remote. I knew he was very famous; I began to read some of his papers. I read his paper on the Compton effect, you know, this simple thing, collision theory, and I think I knew a little bit already about his theory of specific heat. But in the summer of 1925 I

took a course — we could take "equivalent courses" at the University — and there was this man Bar. He gave what you would call now "Advanced Laboratory," and I was the only man who registered for that course. He was a Privatdozent then; he wasn't even a full professor. [Short interruption]

Kuhn:

You started to tell me about Bar and this course.

Bloch:

It was an advanced laboratory, and he more or less let me do whatever I wanted. I had at that time read a little bit about the Millikan experiment, so he said, "Fine, you do the Millikan experiment, the oil-drop experiment." And I did that and set up the chamber, and he gave me the gamma source and I looked at it in the dark-field microscope and came out more or less with a number for the electric charge. But the main thing was that I — I mean what this man did to me — he was an extremely nice man; he died very early, you know. I think he has never been quite recognized for the quality of some very fine work — reflection of light rays from sound waves — I think it was he who did it the first time. It was a

suggestion of Debye's, but he did it. He used the standing sound waves as a sort of grating to reflect light waves. But he gave me extreme encouragement. At that time he gave me extra things to read. [Short interruption] At that time this man Bar apparently had quite a high regard for me because of my obvious interest, and I got strong encouragement from that. I also found at that time that these experiments, although they had been done before, were really interesting. And I was not decided at that time at all whether it was theory or experiment —

Kuhn:

Well, I'm curious because you said you had not liked the experiments very much in Debye's course and had done very few of them, and then you suddenly go over and you elect voluntarily to take an advanced course. Why was that?

Bloch:

Yes, yes. I did and I think it was the personal interest. Well, at that time I felt that maybe some experimental background would be good for me. When I started to take that course, I had this very

great personal attention which I did not have in the big laboratory, in Debye's course. There was that difference. Besides, it was a much more interesting sort of thing to do. During that time I took Debye's course in physics, a very excellent course in mechanics, which was taught by a man [E.] Meissner,...but that's not the Meissner of the Meissner effects. He was for a long time professor. I think he was a specialist in geophysics, earthquake waves and things like that. He was not a too-fatous man, but a very good teacher.

Kuhn:

How far did that go? Did you get Hamilton-Jacobi theory?

Bloch:

No, I don't think; analytical mechanics in that sense was not taught to us. It was rather practical; we had probably Lagrange equations, but in the applications we came to the mechanics of continua — fluids and solids, at least in the outlines.

Kuhn:

Was there a book you used for that?

Bloch:

No. I did rather (effusive) reading at that time, because although the introductory courses were taught very well, they were not all taught. There were big gaps left. For example, in electromagnetic theory I never had a coherent course. We were left to our own resources; I read the book of Abraham on that. And thermodynamics really were never taught. Instead I read the book of (Danke). Who advised me on these books I do not know any more, but somehow I think I must have gotten good advice because these are good books. So I learned that from books. And then I also started to read the book of Sommerfeld; in fact, I think I started to read the book of Sommerfeld first, realized at that time that I would have to know a lot about electromagnetic waves, and I think then went back and read Abraham and then came back again to Sommerfeld. And of course, the reading of Sommerfeld, there I realized that this is the sort of thing which is everything, in the foreground of interest.

Kuhn:

When do you suppose you read it?

Bloch:

I would say — I'm not quite sure — either towards the end of my second year or in the beginning of my third year. I don't quite know. It must have been 1925 or early '26. I think, during my third year then, at the time that one began to look for something one might work in, I somehow spoke to Scherrer first. I had read something about band spectra — it involved some calculations — there were some people — there was Manneback, I believe, working with Debye at that time, who had done some calculations. I became at that time acquainted with the more senior people, and I heard from him or about him and somehow thought there was something interesting to be done. I suggested it to Scherrer, and he said, "Fine. Why don't you try to measure some band spectra? We have a quartz spectrograph in the ultraviolet. Why don't you set it up?" I got this beautiful quartz prism and played around with the quartz spectrograph and started to set it up; I didn't quite know how to go about it. I went to Victor Henri [at the University] who was a physical chemist and had done some spectra, and he

gave me some hints, and pointed also out to me that to adjust the spectrograph is a major enterprise, not simple. And I played around a little bit. We took some spectra with an arc source; we got a discharge, got some ultraviolet light from that and took some spectra, but they were never very good. And I became a little tired of it, and somehow drifted away from the experiment and read, as I said, Sommerfeld —

Kuhn:

Did you also at this time consider working with Debye?

Bloch:

This I want to tell you, because here now it starts becoming a little bit serious. I was still — except with Bar, I really had no personal contact with the physicists of Zurich at that time.

Kuhn:

How about with the mathematicians? Were you taking very much mathematics along with this?

Bloch:

No, no, not terribly much. I mean I took what was required; I took a course in calculus; I took theory of complex functions with Polya; again, a very fine teacher. I took the seminar, in which, by the way, von Neumann was also present although he studied chemistry at that time, but he sat in that seminar. It was a very advanced seminar, together with Weyl and Polya, on complex functions. I did some studying, but I don't think that I was. Weyl and Polya knew me; I was one of the students, but I do not believe that I made any shiny mark there. There was another man, Bohnenblust, who is now a mathematician down at Cal. Tech. — he was far superior in mathematics in my generation. Then, of course, there was Neumann, who always knew everything anyhow.

Kuhn:

I asked you this because when you begin to publish, and then particularly in your thesis, you use some fairly elaborate mathematical techniques; and I wondered where this came from.

Bloch:

I tell you... I did a good deal of reading. Once I had, I think measles — no, no, something else — jaundice, nothing very serious, and I had to lie in bed. During that time I read the book of Courant-Hilbert. I think that's where I really learned my mathematics. I did not only read it while I was in bed; I worked — incidentally, I should say that, of course, was something I did already during the Gymnasium, when I was in bed, also. There was nothing else to do; I constantly did calculations, applied analytic geometry — whatever I had learned — to all kinds of things. This I have done for a long time. So my acquaintance with mathematics was good, but I believe what I needed for my first papers I think I essentially learned from Courant-Hilbert.

Kuhn:

You're one of the first persons who have said to me, "I not only read books, but I used to do applications, calculations in my —" That seems to me terribly important.

Bloch:

Oh, I'm sure that I owe my proficiency at a very early stage only to that. I did it as a game, just because I felt this is more entertaining than to solve crossword puzzles. In '26, or early '27 — I don't quite know — I had an idea. Yes, I read this paper of Debye on the Compton effect. Having read a little bit about Sommerfeld, I realized there was an assumption which Debye made in his paper. He takes an electron at rest. Then he says, "Now I come to the collision with light quantum, and I will calculate its momentum and thereby the momentum and energy of the scattered light quantum." And I said, "Well, there's something wrong here; after all, the electrons are not at rest. I just read in Sommerfeld that they are in motion." And I thought a little bit about it, just simply took the Debye formula but started with a moving electron and realized that this had some effect, at least, on the scattered light. (I went, rather proud, in) to Debye. I screwed up my courage and went to him, and he was rather nice. I said, "I've done that." And Debye said to me, "Yes, well, that may all be quite amusing, but you know, this is not at all any more what people think about the atom. This is all old-fashioned. You

should learn about the new mathematics." Now, wait a minute; I'm sorry. This happened before. I should tell you about this famous colloquium... I wish I knew exactly when that colloquium was. It must have been in (???) I believe. Do you know when Schrodinger's papers were published?

Kuhn:

Yes, '26.

Bloch:

'26. Well, it was shortly before that time. I tell you what I remember about this colloquium. Yes, I did already go to the colloquia at that time.

Kuhn:

I think from something we said in Copenhagen, there were probably two colloquia here that we need to talk about.

Bloch:

Yes. Two.

Kuhn:

One of them involved particularly the de Broglie paper. Why don't you start by telling me at what point did you start going to these colloquia?

Bloch:

I don't quite know. I might already have gone in my third or fourth semesters, but certainly in my fifth.

Kuhn:

Was this a joint colloquium for the University and the ETH?

Bloch:

Actually the colloquium was held at the University building further down there. I went there and, as I said, quite often did not understand —

Kuhn:

How big a group was generally there?

Bloch:

I would say not more than thirty.

Kuhn:

Thirty.

Bloch:

Between twenty and thirty people. This included some few students, assistants and professors, both from the ETH and the University. I don't know whether there were that many. In this first colloquium — now wait a minute. I seem remember something else, but here I'm not quite sure — that in one of the colloquia, Debye asked Schrodinger whether he could not report about the thesis of de Broglie. He said he would like to hear about that sometime. I believe I remember that. I certainly remember very vividly that Schrodinger did speak about the theory of de Broglie, essentially explaining in a very clear way but not adding anything new — just this idea of de Broglie that there might be a general relationship between momentum and wave length and that the stationary orbits could be explained by simply counting off an integral number of wave lengths along the orbit.

Kuhn:

Let me ask you. Your recollection is that he did do that derivation of the Sommerfeld conditions?

Bloch:

I think so, yes.

Kuhn:

In de Broglie's paper, as a whole, that's a small point in a very much longer paper. I'm curious to know whether he really selected that out.

Bloch:

I am not sure. This may be retrospective. I would not want to swear to that. I have a feeling it was essentially a fair reproduction of de Broglie's basic ideas to the extent that when I later read de Broglie's thesis I had not too great difficulties in understanding, and that was surely due to Schrodinger. Then, and this I am quite sure of, at the end when Schrodinger was through, there were of course some questions asked. But Debye simply made the remark and said that this sounded rather naive to him; I don't know whether he used the word

"kindisch" or "childish" or something like that — "naive". He was under the impression or had learned — I don't want to put too many embellishments in because I've told this story many times, and as one keeps on repeating it, it gets better. But the gist of it is that he felt, quite naturally, that when one talks about wave phenomena, this should be based on a discussion of the wave equation. I understand much later that this, of course, is clear because Debye came from Sommerfeld's school and, of course, they were brought up this way. And that was all.

Kuhn:

Do you have any notion when this happened?

Bloch:

No, no, I don't remember. If I wanted to find out, I would do exactly what you do now. I would find out when Schrodinger's paper was published —

Kuhn:

This was before Schrodinger's —

Bloch:

That was before, but not much before. I would say maybe three months before or so.

Kuhn:

Perhaps not. One of the stories is — again I've got no direct information on it, but enough people know it so that I think it's true — that at some time before the publishable papers were done, Schrodinger had a relativistic wave equation; and that he put this away because the results came out badly, and it was only later that he came back and did the non-relativistic one.

Bloch:

As far as I remember, the time-dependent Schrodinger equation was not terribly important at that time. I think he used the time-independent Schrodinger equation to get the energy levels.

Kuhn:

In fact in the first three papers he's got a time-dependent Schrodinger equation with a second time-derivative, which he only later changes.

Bloch:

Yes, but I mean his real results he got from the time-independent because he wanted to get eigenfunctions and eigenvalues. To my recollection it cannot have been more than a few weeks after this de Broglie seminar — I think it was only two weeks later, but it might have been three — that Schrodinger said that Debye had suggested to him this idea of a Schrodinger equation and he had it, a wave equation. Don't ask me too many details because I was a very young student at that time.. It is quite possible that he started with a wave equation of second order like one does in optics —

Kuhn:

He lectured on this, you think, shortly after — on the wave equation. Did he produce some applications?

Bloch:

I think, but again I'm not quite sure; I think he had already the energy level of the hydrogen atom. And there I know he got a very great contribution from Weyl, because he didn't know too much about eigenvalue problems of differential equations. Now I

cannot quite swear to that, but I was certainly impressed at that time by the shortness of time interval between the first suggestion of Debye and the fact that Schrodinger had an equation and had results.

Kuhn:

That's terribly interesting. It puzzles me because, as I understand it, there is an intervening stage in which he has a relativistic equation and results that are not coming out right. He's got the relativistic equation with no spin terms.

Bloch:

I don't recall that phase. If it was there, I'm sure it was of very short duration.

Kuhn:

That may well be.

Bloch:

Because it did not take him long to get the right equation. He had realized then at a very early stage the connection between the wave equation and the

Hamilton-Jacobi equation, which is of course first-order in time. I think he latched on that quite early.

Kuhn:

Do you remember at all how he derived his wave equation when he first showed it to you? Because in the published papers it goes through various stages.

Bloch:

No, no, I do not remember that. I do not remember.

Kuhn:

There is another story about a remark of Debye's at about this same time. I wonder whether you have any recollection of that, which was that Debye, after first seeing the way Schrodinger handled the wave equation, said something to him about it—that "if it's a wave equation, you ought to be able to derive it from a variational principle."

Bloch:

That I do not recall, no. That I do not recall.

Kuhn:

How did you, and how did Debye, and how did others feel about the Schrodinger equation?

Bloch:

I do not believe that I understood at that time its importance. And I'm not quite sure that Debye did. Debye probably felt that it was rather trivial. That there was something exciting and new coming on I think was not clear to me until about the summer of '27. The first thing I remember there was the question about the significance of the wave function. Schrodinger's ideas that the Ψ^2 ; He did not like this idea, never liked the statistical interpretation. He thought that the wave packet which one could form represented the actual particle and its shape. I think it goes back to that time that I began to realize that something very fundamental was at stake — not before that.

Kuhn:

Had Schrodinger lectured on this outside of the colloquium? Had you seen more of him after this — ?

Bloch:

Yes, I had. This is what I wanted to say, and I think here my chronology is not quite right. There was this colloquium which I told you about—these two colloquia. I believe my visit to Debye concerning the Compton effect was either shortly before or shortly afterwards — probably shortly afterwards — because when I came to him with this idea, he said to me, "Don't bother about this old-fashioned kind of atomic mechanics. Learn the new quantum mechanics." And this of course I did learn; I learned it to a very large extent by reading Schrodinger's papers.

Kuhn:

Let me simply raise a point with you. If it is before the colloquium then he can scarcely have meant —

Bloch:

That is not so certain, because of course there was Heisenberg's.

Kuhn:

Yes. what I wondered, you see.

Bloch:

However, I do believe — as I say, I'm not quite sure — but I do believe that when Debye made this remark to me, he did not mean for me to study matrix mechanics, but he meant for me to study wave mechanics, which is in fact what I did.

Kuhn:

Were you involved with Heisenberg's mechanics, at any point?

Bloch:

No. Later on. Later on, but not at that time. No, I think I didn't take that seriously up until when I came to Leipzig. So as I say, I'm unfortunately not quite sure where to place this incident with the Compton effect. I rather think now it must have been after. I think wave mechanics was already in the coming and Debye was sufficiently convinced that that was the right way to do it and that these quantized orbits were out. He gave me this very excellent advice that I should study that. Then I did get into contact with Schrodinger — personal contact. Also what happened very much at that time

was that Heitler and London came; this was in '26. They came in '26, and they were very nice. Although I was only a young student I established at that time a very close friendship, and they took me on walks, and sort of told me they were working at that time on the covalent-bond theory. I think I began already to understand what it was about, and they helped me, explained it. So I got quite a bit of stimulus from them during the time when —. Of course, during that time I studied — this must have been in early '27. I read the papers of de Broglie and of Schrodinger and began to understand, really, wave mechanics and the Schrodinger equation. Then in the spring of 1927 I came to Schrodinger. I had heard, either through him or from London or from Heitler, about this difficulty with the wave packets; and I had an idea which I thought was very good.

Kuhn:

When you say "this difficulty," tell me what you mean.

Bloch:

The interpretation of the Ψ^2 . And the fact that wave packets ran which bothered Schrodinger very much.

So I thought I could help him. went to Schrodinger and I said to him, "Look," — of course, you will be amazed how naive I was — "so far people have always spoken about the motion of electrons in an atom, but they haven't taken radiation damping into account. Maybe radiation damping will hold all the waves together." And believe it or not, Schrodinger said, "That's a very good idea. Why don't you try it?" It was a very lousy idea. That dialogue with him was the reason why I took up the problem of radiation damping. Actually, I did not yet know Dirac's —. I'm not quite sure, because part of this first paper on radiation damping I had already before I came to Heisenberg. I must have treated radiation damping in a somewhat more globally. I do remember that the main result came out right. I did it for a harmonic oscillator, and I could show why the harmonic oscillator had the nice property of staying together anyway. With damping, it not only stayed together, but the amplitude actually died out. This I was rather proud of. Beyond that I didn't go. Then came a great crisis in Zurich because at the same time Schrodinger, Debye, and Weyl all left.

Kuhn:

I didn't realize that Weyl had left at the same time.

Bloch:

Yes. He also. Weyl went to Gottingen, Schrodinger went to Berlin, and Debye went to Leipzig. And then it was clear to me — I was about to start my thesis, and everybody with whom I would have cared to work left. I talked to Debye about this problem, and he was very nice. Oh yes, incidentally, some time during that time Scherrer once came to me and said, "What's happening with your experiment down in the basement? I haven't seen any changes for several weeks." And I said, "Yes. Well, I now study quantum mechanics." He said, "That's fine, as long as you do anything at all." And that was the end of my experimental career, at least temporarily. I spoke to Debye at the time, and he decided to go to Leipzig, which was probably the spring of '27. He suggested to me that I should go to Leipzig and study with Heisenberg. I don't quite know why I took his advice, of course, but I had heard about Heisenberg; I respected Debye very much. Maybe I also thought I'd better go to a place

where I knew at least already one of the professors. Anyhow, so then I went in the fall of '27 to Leipzig...

Kuhn:

Before we begin with Leipzig I should like to learn more about how you and others felt about the interpretation problem and the wave-packet problem while you were still in Zurich with Schrodinger.

Bloch:

I was too naive to appreciate the depth of the problem. Probably more or less on authority, namely on Schrodinger's authority, I also thought that there was apparently something missing in the theory because of the very fact that this ought to stay together — an electron doesn't become a mile long. And so something must have been forgotten. That there is a statistical interpretation I do not think was pointed out to me very clearly. I'm not quite sure to what extent even London and Heitler knew about it or were convinced of it. Have you spoken to Heitler about that?

Kuhn:

His memory was fairly vague on this.

Bloch:

On this point of the interpretation of the wave.

Kuhn:

Yes. He had been at Gottingen. I think the chances are good that he was already working on own interpretation. But I'm not sure.

Bloch:

I do not remember that in my discussion with Heitler and London this played a great role. And therefore I do not believe that it stood in their foreground of interest particularly. They probably didn't even care very much about it because all they were interested in at that time were the energy levels, and they were very much intrigued about these two symmetry classes, one leading to attraction the other to repulsion — they didn't need really all this. And if they thought it was an important problem, they certainly didn't bring it to my attention. I must say I was not aware of the problem, except that I realized — this much I understood — that wave packets have a tendency of running apart, and then I had the idea maybe I could keep them together.

Kuhn:

Were you worried or were people in Zurich worried about some of the other problems that get emphasized in this respect? For example, the fact that transition probabilities should be proportional in the Schrodinger theory to the intensity in both the initial and the final state? You know, he viewed radiation as a beat phenomenon between two levels.

Bloch:

Yes. I did not speak to people about that. It seemed to me at that time very natural, and I did not meditate on that. I just simply said, "Well, that is a rule, and it works." I was not mature enough — I was only twenty-one at that time — I was not mature enough simply to appreciate the depth of the problem. I was pleased. I said, "Well, fine. There is a new mathematics —"

Kuhn:

Do you really think that's a matter of maturity? I don't mean now that maturity is not also involved, but isn't it also true that there are different sorts of physicists? That some would say, "Here is a rule that

works, and I will not —." Wasn't Debye a very good example of a man who very often brushed aside the deeper problems and said there were no problems there at all?

Bloch:

Quite, quite.

Kuhn:

He was also a very mature scientist.

Bloch:

Well, mature, perhaps so. Yes, all right. Not philosophically minded., let me put it that way. My attitude was, of course, [determined] through the influence of Debye very much at that time. I mean nobody pushed me into these philosophical problems; I didn't care. I felt, "Well, there are nice things coming out, and let's see what's coming out of it." That there is something profound in it I had a first inkling of, but only an inkling, under Heisenberg, and to the real things my eyes didn't become opened except through Bohr. That was much later. In fact I had some reluctance at the beginning, a typical Debye attitude. by speculate

about these things we know how to calculate? Only much later I realized that. So at that time there was talk, I think, in Zurich that Schrodinger had some idea and people didn't see with them, and it seemed more likely that Ψ^2 should be a probability. I don't know, I must have shrugged my shoulders to that, or less. So I was really, shall we say, rather indifferent... [short interruption] ... to the interpretation of the wave function. Coincidentally, I think I know quite well now how I did this radiation problem that time, because I think all I did, since Schrodinger had already calculated transition probabilities, was simply to say, 'All right, there are certain transition probabilities which go from one state to the other.' And I simply took these into account as damping in the wave functions and then built them up to wave packets and noticed that the whole wave packet is damped. That's how it was.

Kuhn:

You went off at this point, then, to Leipzig?

Bloch:

Yes, that's right. In the fall of 1927 I went to Leipzig. I had something written up about that. In

fact, I had reported at some minor meeting, I think, in Zurich or somewhere or some place. When I came to Leipzig Heisenberg wasn't there yet. I knew he was about to come. Only Wentzel was there. He was extremely kind. I showed him that paper, and he looked at it and he said, well, he didn't feel very competent about these matters, and he suggested that I should show it to Heisenberg. Beyond that, we didn't really go very far. It was only a week or two before Heisenberg came back. Then I think Wentzel must already have mentioned it to Heisenberg or — I don't quite know. Anyhow, when I saw Heisenberg, I told him about it. He listened to me, and he immediately discarded it. I don't know, I think I realized already at that time that this isn't going to do anything to wave packets. I mean, he discarded that; he said, "No, no. That's not so." He made me feel, "This is quite clear. This is a probability, and don't worry about it any more." And in my unphilosophical attitude I accepted that. However, he did say that the problem would interest him, that it should be done more thoroughly, more generally, not only for the harmonic oscillator, but generally speaking. I don't know whether he said —. Yes, I suppose he said to me that I should really use

Dirac's radiation theory, which was developed at that time. I think I knew already what he was talking about; anyway I studied Dirac's paper and then published this first paper on radiation damping [paper No. 2]. This is more general — in fact, it showed something which pleased Heisenberg very much, namely that if radiation damping does anything — of course, it doesn't do much anyway — it tends to even destroy phase relations. That is to say, the wave packets run even slightly more apart. And that was that. I can say that was finished.

Kuhn:

... Coming to Leipzig from Zurich, and coming to Heisenberg in particular, you may have gotten suddenly quite a different impression of what was going on in the field. Did this change things for you a lot?

Bloch:

No, I have a feeling that the transition was rather gradual. You talk again about the interpretation?

Kuhn:

Let's say clearly the interpretation would be one of the issues, but I wonder what Heisenberg's attitude was toward the whole Schrodinger approach still? This is still '27.

Bloch:

Oh, very positive. Schrodinger [sic i.e. Heisenberg] had already himself used the Schrodinger equation to explain the ortho-para hydrogen. No, no. I mean, as a technique he thought it was fine. Heisenberg did by no means, for example, tell me at that time, "Well, you'd better study some matrix mechanics." Heisenberg was not a man who cared about methods. He felt that the Schrodinger equation was a very convenient method. Incidentally, of course, through Schrodinger's paper in which he established the connection — which we had read by that time — with matrix mechanics, I knew enough about matrix mechanics. Heisenberg fully realized at that time, as I think everybody did, that the two methods were equivalent. And Heisenberg was not a man of narrow prejudices, I mean. Whatever worked, was fine. So he was very pleased that I did know

something about the Schrodinger equation, and that that was equivalent to the matrix mechanics I knew. The interpretation, as I said, didn't bother me very much, except that Heisenberg told me simply, in a very quiet way, "No, of course this is not going to do anything, but it is an interesting problem." I immediately realized, without any regret, that this idea of the holding over of wave packets would go, and I evidently felt, "Well, the great scholars don't see any particular need for it, but there is some point at least in going into radiation damping, because it hasn't been done before." I should say I had a tremendous impression of Heisenberg, right from the beginning. I mean we struck it off very well; he was extremely kind to me. I was his first student so I had a lot of time; I participated immediately in the seminars and established a very close and good connection with Heisenberg and admired him tremendously. Consequently, a statement like that I would of course take. It was of course true, and I had the feeling, "Well, that settles it. There's no use worrying about that any longer." Heisenberg himself was not too much interested at that time in questions of epistemology. I think he was quite happy about his gamma-ray microscope, and that answered the

question as far as he was concerned. He [had] also a little bit of this sort of aggressive way, like saying, "Well, now we have a new —. Let's try to understand metals and this and that."

Kuhn:

When he got there, he had recently been both at Como and then at Brussels at the Solvay Conference, where these had been very live issues. Was there any carry-over that you remember —?

Bloch:

Not that he transmitted to me, no.

Kuhn:

Would you say that it was not only you and he, but that in practice at Leipzig, even that early, these interpretation things were already pretty much dead issues?

Bloch:

I think so. Yes, yes. There was no great discussion. Really, I must say that there were problems and deep problems I wasn't even aware of, except through rumours occasionally, until I came to Copenhagen.

Kuhn:

That fits much with the impression that I had had. Until one's heard it three times, one is never sure, in view of the nature of memory, that it's going to come out the same way.

Bloch:

I must also say, although at that time I began to know several of the younger generation — let's see, soon afterwards Teller came, and Weizsäcker was there, and I mentioned already Heitler and London — none of these people, at least at that time, seemed to be particularly excited. by the interpretation. This was the Copenhagen spirit that had not yet particularly penetrated. I don't know, I may be unjust, but I have the feeling that even Heisenberg felt, well, it was an interesting question but not all that interesting. Heisenberg was himself quite young at that time and not really philosophically minded. I think he was quite —. He was really more interested in seeing what consequences one could draw from the theory than to —. I mean, for his taste, the $\Delta p \Delta q$ settled the business.

Kuhn:

What problems was he working on? How did you get to the theory of metals?

Bloch:

Now, let me see. Well, that was his suggestion. He was interested in sort of down-to-earth applications. After I had finished this paper and written it — that was in the winter or late fall of '27 — I went home; and he said to me at that time, "Well, I think you ought to think now about a thesis problem. Why don't you think about some questions, what you might do?" I came back with some rather poor suggestions; I was not very educated at that time. I thought, for example, that it might be of interest to follow Ehrenfest's adiabatic theorem through, see how it goes in quantum mechanics. That question apparently was so clear to Heisenberg that he said, "No, I don't think that's very interesting."

Kuhn:

Wentzel did that too, didn't he?

Bloch:

I don't remember. Not at that time. Heisenberg felt that it was a purely formal problem because he could quite clearly then see how in an adiabatic transformation the states in quantum mechanics go into one another. Well, he suggested two problems that show very clearly what his interest was. One was: Sommerfeld had written before papers on the conduction electrons and had pointed out that the Fermi statistics give the right behavior in experiments on specific heat and some of the thermoelectric effects, and so it was clear that there was more to be done about that. Heisenberg said to me, sort of in a gentle way, "Well, maybe one should see whether the electron theory of metals, what that becomes like in quantum mechanics." That was one possibility. And the other he mentioned to me was the idea of ferromagnetism. I knew very little about magnetism at that time, but he mentioned to me —. He said, "Well, there is that Weiss field, and it strikes me as very similar to the problem of ortho and para helium." That is to say that the para position has really to do with the symmetry and therefore it's an electric force. He sketched out to me briefly why that field is so very great because it's not

really of magnetic origin. So that was the gist of this problem. But I think I felt, "Well, Heisenberg has it already in a nutshell." That was his first idea. And then I said, "I don't want to just simply work it out." Besides, I didn't probably realize at that time that this was a problem of major importance: 'Well, very nice. So OK. One can't explain the Weiss field, so what.' So Heisenberg did that himself. But I was then interested in this question of metal electrons, and very soon — in fact, I think only a week or two after, he told me that — in some primitive way I had gotten the essential idea that a periodic arrangement is not really an obstacle for waves, but it's only the thermal vibrations. Heisenberg was very pleased. I told it to him only in the one-dimensional case, in a very primitive way. And he said, "That is explained now. Now I understand."

Kuhn:

Did the problem you set yourself or that Heisenberg set for you include the notion of getting the potential back in? I mean this Sommerfeld had not done.

Bloch:

No, no. No such thing. It's just that, in a general way — after all, Sommerfeld had only applied the statistics — but there is more to it. Maybe it's conduction; maybe it's a question of metallic resistance. It ought to be treated properly. And that's clearly it. More than that he didn't say.

Kuhn:

In the first of the published papers you bring in group theory very quickly.

Bloch:

I did that. That was the fashion. That was my tribute to fashion. I did not want —

Kuhn:

— I was going to say, you helped to establish that fashion!

Bloch:

No.

Kuhn:

I don't mean that you invented group theory —

Bloch:

No, no, but the application of group theory came from me.

Kuhn:

I don't know an earlier case where you're considering linear displacements.

Bloch:

Well, the most trivial of all groups, you see.

Kuhn:

But, still and all I would say that at this time an awful lot of people are still saying, don't have to learn that sort of mathematics." Heisenberg wasn't using group theory that early. He's also not failing to solve problems because he isn't using it.

Bloch:

Well, I mean it was talked around at that time that group theory is very profound — a lot of mysticism was made about it.

Kuhn:

Now, already —? Where was this going on?

Bloch:

Oh, I should say this: we had a very lively communication with Berlin. We were quite frequently at colloquia in Berlin. The whole group — Heisenberg and Wentzel and the students — went up to Berlin. So, no wonder! That was probably there somewhere in the Berlin atmosphere. Von Neumann was there; we knew him well from Zurich, and he mentioned it to me and talked to me about that.

Kuhn:

Had you really gotten to know him fairly well in Zurich?

Bloch:

Not too well, Well, you know, a fellow sits at the next bench; I knew he was a genius; that was quite well known.

Kuhn:

Was that generally known already of Neumann right from the beginning?

Bloch:

Among the "ins" it was known. Weyl had a very tremendous respect for him, and I could see in this advanced, seminar when Weyl didn't know the answer he would say, "Neumann, how does that go?" We all realized this was a great mathematician. Then I met him again in Berlin. He was extremely kind, and I was at his house; and I think through conversations with him — I knew Neumann more than Wigner at that time. I had the impression that group theory is something tremendously important. Later on, I didn't think so much of it any more, but at that time I did. And the idea of why a periodic potential doesn't really present an obstacle to a wave —. I mean I didn't know anything. Of course, if I'd

known about periodic potentials — the work of this astronomer — I would have known. But, anyhow, I did it in an extremely primitive way. All I did was that I said, "Well, I'll make a Fourier analysis; if it is periodic, then I'll make a Fourier analysis of the waves also." Then I realized that nothing is happening; in fact by putting the Fourier components together, I realized that it's a wave which is modulated. That's what I told Heisenberg, just in that primitive way, for a one-dimensional lattice.

Kuhn:

Aha! So the whole group theory comes in later as a way of doing this —

Bloch:

That's a tribute to the fashion. It was simply that I felt at that time that whatever you can do with group theory you ought to do. It is not the way I started into it. And. I think it was my first and last application of group theory. [Chuckle].

Kuhn:

It's a neat one. It's a very nice, general proof.

Bloch:

Anyway, that's not the way I did it. And so this was the first thing. Then Heisenberg felt — well, he was very pleased and said that was clearly the key to the whole question of what one —. And I may say this much: that although Heisenberg was a tremendous stimulus, gave the problem himself to me and was always very encouraging, the way how to go at it I think was really my own. That one first had to understand what happened in this strictly, purely periodic potential was clear to me. It was clear that Sommerfeld simply considered it as if there were no ions at all, and I knew, of course, there were these ions. So that was it. From then on I went pretty much on my own, with occasional help and correction from Heisenberg. I would say I saw Heisenberg regularly. Well, I saw him often in the seminars and so on, but also occasionally he said, "how is it going?" Sometimes I had difficulties, and Heisenberg made a suggestion to me: "why don't you try it this way?" And, very often that was it. So I wrote this paper on the conduction of electrons in a very short time; I think that in about half a year it was finished. I made a mistake there, by the way. You don't know these things so well. I came out, for

high temperature, with the right law, linear with temperature. But for the low temperature the law was wrong; I had a T^3 law. This was a sloppy discussion of an integral equation which Heisenberg didn't catch because Heisenberg wasn't much of a mathematician, ever. [Chuckling]. He believed that to be —. He shouldn't have believed it. I did that the next year, a year later, maybe two years later. Then I corrected it; that was T^5 there.

Kuhn:

Did you go over it yourself and find, the mistake yourself, or did somebody call it to your attention?

Bloch:

That I don't quite recall. That I don't quite recall. I had the feeling, probably, at that time, that I'd pulled some swindle there. I wasn't very sure of it. It was one of those tricky cases where, in the limit —. Because, neglecting certain terms, I got an integral equation of which the homogeneous equation had, a solution, I sort of glossed over that. Later on it occurred to me. I knew one had to be very careful in this case. Then I realized, 'If I keep certain terms, it doesn't have a solution. At that point they must be

essential.' They were small terms, but they were important because they made the difference whether the determinant is zero or finite. That sort of thing. I think I had a bad conscience at that time. Actually, now, I don't think anybody put me [on] to it. But 'then shortly afterwards I met Grüneisen. I'm going ahead of my time; that comes later. This was then my first year in Leipzig. I don't believe — except that I learned an awful lot from Heisenberg, had talks with him. He did, of course, at that time work out a theory of ferromagnetism. I learned it directly, first time, from him. He began to work already with Pauli on the quantum electrodynamics and told me occasionally about the work. He was extremely optimistic. He felt, well, of course, now one has to quantize the electromagnetic field. He didn't foresee any difficulty there. He just said, "Well, everything will come in order now. Clearly one must just carry this program which Dirac has really started, must carry it through; and everything will will come out fine." I do recall that he came to me occasionally because he was in constant correspondence with Pauli. What usually happened is that Pauli pointed out certain difficulties to him, and then Heisenberg had solutions. For example, this business with

divergence of is equal to lap. But this is really a boundary condition, you know. And. Pauli pointed out that difficulty to him. Then Heisenberg had some way, a rather 'dirty' way I think, of getting over that difficulty. I swear they somehow, by-passed it. They didn't have the neat way that Fermi later had. I just remember that much of the time, and. I do remember the extreme optimism of Heisenberg. He thought that all the difficulties were going to disappear. How soon he realized —. I think that was later, when I was already gone from Leipzig.

Kuhn:

Was anybody else in Leipzig at that point also interested in the field quantization problem?

Bloch:

I don't think so. There were very few people in Leipzig. During that first year, I'm not quite sure —. Peierls was already there, yes; but I think he was only a student. He was not sufficiently advanced.

Kuhn:

You didn't get involved at all with field quantization, did you?

Bloch:

No, no.

Kuhn:

Did, you follow even the papers that were coming out then?

Bloch:

I think so, yes. I think so. I did a lot of reading. I read Dirac's paper at that time.

Kuhn:

What about the Jordan-Klein and the Jordan-Wigner papers?

Bloch:

... I knew about them, but I don't believe I used them, nor did. I probably at that time appreciate their importance. I mean, I felt, well, that's very nice; instead of working in the space of many dimensions, you can do it this way. It sounded rather complicated to me and didn't seem to offer anything new. In fact, it didn't, you know. It's not really until Fermi used these as creation and destruction operators that the

real depth of it came out. It seemed to me a formal device... I don't believe that Heisenberg was terribly much interested. Well, he knew about it, and he said, "Yes, one can do it that way." Transformation theory I think I studied at that time; that rather interested me. Yes, that I knew. I have a feeling that at that time I paid really more attention to Dirac's papers than to the Gottingen school.

Kuhn:

You were there when the Dirac electron came out, weren't you?

Bloch:

Yes, yes.

Kuhn:

What sort of an impression did that one make?

Bloch:

That I can tell you. This was very interesting. Heisenberg told me about it and said, "Dirac had the relativistic Schrodinger equation where the spin came out." And then I read it. I came afterwards to him and I said to him, "It sounded rather obvious to

me." Heisenberg said, "Yes, one thinks that always afterwards." There I had the feeling that was a naive, cocky approach; and Heisenberg told me quite plainly, "Well, this is more an achievement than you realize." I was not very much impressed. I just said, all right, so what? I knew that a simultaneous system of several first-order equations gives you a higher-order equation.

Kuhn:

You're the first person I've ever met who reacted to that that way.

Bloch:

Maybe I'm the first honest person —. I thought it was trivial. Heisenberg pointed out to me that there was more to it than meets the eye.

Kuhn:

Was it trivial that, going at this from the point of view of a relativistically invariant wave equation, one should come out with the spin matrices without ever putting in anything about spin?

Bloch:

It didn't impress me very much, I must say. The whole reasoning of Dirac sounded so misleadingly obvious when he said, "After all, the Schrodinger equation must be first-order in time." Then he said, "How do we get [that] out? Naturally, by simultaneous equations. But I can't do it with two, so I need four." That was too bad, but all right. That the spin came out — No, I think that was really not too surprising, because that the spin was a relativistic phenomenon was, of course, well-known. And, that came probably from Sommerfeld via Heisenberg to me. After all, Sommerfeld had had a derivation of the fine structure as a relativistic effect. The effect of the spin was just of that nature. And that it was a relativistic effect was clear. Well, as I say, my reaction to that was evidently quite unjustified, but I do remember that I didn't have the slightest doubt it was right. As I say, I thought that was a rather simple —. And Heisenberg corrected me on that. It was really a year of tremendous growth because really I think that at the end of my Leipzig period, when I got my Ph.D., though there were still very great gaps, I think I was essentially at that time a physicist. I think I knew the more important things

which were going on in physics, except for the epistemological things, which I just paid no attention to. I had also a transitory period quite near the beginning, where I was interested in this five-dimensional theory of Klein and Kaluza. That attracted me for a while, and Heisenberg said, all right —

Kuhn:

In this year?

Bloch:

Just after I came, after the radiation paper or during the radiation paper, I participated in a seminar; and, since I was interested in these things, Heisenberg suggested once that maybe I would give a talk about it, which I did. But Heisenberg was not very much interested in this sort of thing; he felt it was a rather formal approach and directed me — and I was very willing to be directed in that direction — toward more down-to-earth things. He didn't want me to get lost in this.

Kuhn:

No. Where had you gotten the relativity theory for that

Bloch:

That I studied on my own, as a student. Oh, that I can tell you. There my source of information was Pauli's Encyclopedia article; that I read in my second or third year. And again — I read it, but read it with a pencil and paper. I must say I really know my relativity from there, and. I. know it well.... Well, I think I'm essentially through with my Leipzig period. I had three periods in Leipzig. And I'm telling you about the first.

Kuhn:

Three I didn't know. I knew only the two. This first one was simply '27- '28?

Bloch:

Right. Then I went to Pauli. I can just tell you in a nutshell... '28- '29 I was with Pauli; '29-'30 I was in Holland with Kramers. Then I came back to Leipzig, but as an assistant, and was in Leipzig 1930-31.

Kuhn:

The full year?

Bloch:

The full year, — or was it? — yes, full year. Then I went to Copenhagen, for half a year, till the spring of '32. And then I was one more year in Leipzig, until the spring of '33. This is it.

Kuhn:

And then from there to Rome?

Bloch:

Then I went to Rome. But then I spent a summer, after I left Leipzig in the spring of '33, left Germany, I spent a summer partly in Zurich at home and partly traveling around visiting again. I was in Paris and visited again Kramers. Then I went to Rome for half a year, and then I came here. That's it. You want to ask about this first Leipzig period or about later ones?

Kuhn:

I have no other questions to ask about the very first Leipzig period, unless you have other things that you remember about it. The questions that were on my mind, that I had to ask, you have answered. I would say, though, why don't we stay chronological? We will come back there when you get back there.

Bloch:

OK. So first I went —. Well, I mean Heisenberg recommended me to Pauli. I was an assistant. I must say that I went to Zurich with some misgivings; I was afraid of Pauli. I'd met him before, and I knew his sharp, critical tongue; and I was afraid, and Heisenberg thought that was —. He laughed it off.

Kuhn:

Were you his first assistant at Zurich?

Bloch:

No. Kronig was. Kronig had been there before me . And. Kronig, I think, had gone back to Holland. Anyway, I was his second assistant. Kronig, I think, had, been there half a year. And I came then in the

fall of '28; I was his assistant. I didn't have much to do as an assistant. I corrected some papers of his students. Research —. He said, "Yes, well of course the normal theory of conductivity, that's fine; but the real interesting problem in this field is superconductivity. Why don't you work on that?" And I did work on that, but unsuccessfully, for a long time. It was rather interesting because I —. First, I may say one thing. There was an idea about superconductivity which Landau and I had independently. And, that was the basic idea which Pauli immediately accepted: namely, this immense stability of superconductive currents cannot be just a selection principle, some very highly forbidden transition, because nothing in the — world is that highly forbidden. It must be — and I compared it because I knew the theory of ferromagnetism of Heisenberg — compared to magnetism. Landau and I independently. Because the reason, of course, why you have spontaneous magnetization is that this is the lowest energy state. And so for the same reason we felt that there must be an energy minimum, connected with a current. The strange thing is that in a superconductor an energy minimum is a current. Now, this is the basic idea Pauli accepted, and he

said, "Yes, of course. That's the explanation," which I think it still basically is. And it sounded rather simple to say, "All right, now you just have to take interaction into account, and it'll all come." I did that; it was all wrong. And I produced theories at the rate of about one every few weeks, and it took Pauli usually between five and ten minutes to see the flaw in it and send me home. Pauli got rather angry at me, and annoyed: "This is a simple problem. Why don't you —. You're always making these mistakes!" he said to me.

Kuhn:

What sort of mistakes would he find, in the mathematics?

Bloch:

Yes, yes.

Kuhn:

Places where you had persuaded yourself that you would get a current term that he —?

Bloch:

Yes, exactly. And then he said, "Well, that I don't believe. You have neglected this term; I'm sure you can take it into account. I'm not convinced." Or something like that. He was right, you see. And so all the models which I tried were much too simple-minded. That is to say, after I'd done them right, after Pauli had corrected my mistakes, it always came out that at an energy minimum there is also zero current. Very simple-minded models. Once Pauli was apparently not awake, or he felt too well, and he believed it, what I had. So naturally I believed it too, and. I started already to work on the magnetic field. I remember he talked to Otto Stern who was there at that time, and said, "Yes, I think that Bloch knows now the theory of superconductivity. There are some more details about the magnetic field he's worrying about now, but that's not important." They didn't catch me that time. But later on someone caught me, maybe he again. It was also wrong. So I got nowhere. I wrote only one paper at that time. It was a rather unhappy time because Pauli was very critical, and it sort of depressed me. I wrote only one paper at that time, which was of some slight importance because I felt

that the model which Heisenberg — I began then to be interested in ferro-magnetism — that the model which Heisenberg had used for ferromagnetism was not necessarily so. On the other hand, one had this model of metals, of conduction electrons; and I wrote a paper, which I think you have also, on the possibility of ferromagnetism by conduction electrons. This one, yes. 1929, that's right [paper No. 6]. And this [indicating paper No. 5] was a paper that was more or less — yes, this I forgot — an outgrowth of Pauli's paper. I think much of Pauli's contributions I learned at that time, but Pauli had written — well, the so-called temperature-independent paramagnetism. And this was closely related to it, Pauli thought, (???) Pauli, and so it was. So these are the only papers I did write in Zurich.

Kuhn:

You had been in on the Heisenberg end of this correspondence with Pauli about quantum electrodynamics. You must now have seen Pauli's attitude. Did you find that very different?

Bloch:

Yes, yes, very different. Pauli was much more critical about the whole thing. Pauli was in depressive moods at that time anyway, which he communicated to me. He did not feel that things were going well at all. That is to say, he had the feeling that these were deep difficulties. I don't believe that I understood the difficulties. Well, I already realized that there were these divergences which were simply not to be overcome, or not —. I don't quite know. I rather suspect that Pauli knew that, and did not share Heisenberg's optimism. Of course, I do not know whether at that time Heisenberg was still that optimistic. But it was really for me —. The transition from Leipzig to Zurich was to go from optimism to pessimism. And that has partly to do with the character of the people, but I think it has also partly to do with the development of physics at that time, because the early optimism which reigned had not been so (???) began to look rather skeptical. There was one very interesting remark that Pauli made to me at that time, which I remember and therefore it must have impressed me somewhat, although I didn't do anything about it. He was very critical about Dirac; he always was. He

thought that Dirac was a rather shallow thinker, and of course only a man like Pauli has a right to say that. And he didn't believe at all that Dirac's reasoning that the probability density has to be positive was right reasoning. He said to me, "Well, why does he speak about probability density? Why doesn't he speak about charge density? I do not see why charge density couldn't be negative. It can be positive and negative." In other words, Pauli quite clearly realized already at that time what later was to become Weisskopf-Pauli.

Kuhn:

That's very, very interesting.

Bloch:

I had thought at that time, as I told you, that Dirac's paper was trivial, because the reasons seemed to be so straight-forward. And Pauli pointed out to me, just as a side remark, that far from being trivial, it was not at all convincing to him. It's not that he doubted the Dirac equation, but he just didn't see the necessity of it. It's quite clear that he had already the basic ideas of a theory which was positive-definite. This is always what he said. He said, "This is not a

good theory where you have negative energy values. And I think what one should have —." I said, "Yes, of course that's true, but if you want to have the density positive —." He said, "Why should the density be positive? I'd rather have the energies positive, and let the density have both signs." You see. And that's of course what he achieved in the Weisskopf-Pauli theory.

Kuhn:

I had no notion that it went back that far.

Bloch:

I may just tell you — this is a little bit anticipating, but it was quite clear that Pauli had all the basic ideas. Now it is a little bit of a nasty remark, but I think that since you are historians, we have to say it. When I once spoke later to Pauli about Weisskopf and the theory, he said, "Well, probably Weisskopf has done a very nice job; he carried out the mathematics quite competently," or something like that. Now Pauli is not a man who —. Of course, he was always very critical, and it was very difficult to hear from Pauli that somebody was really good. But I think he was right in that sense, that the essence of

the theory he knew. This threw a different light, for me, on the Dirac theory. I think I understood what Pauli meant at that time, but I —. Well, he hadn't carried it out himself, but he evidently realized that if you want to have the one, you can't have the other one and vice versa. That was in 1929. Well, because I was rather depressed at that time, and also I lived at home with my parents again —. That didn't go very well any more because I was too independent and did not want to be the boy anymore at home. So I looked for the first opportunity to get away from Zurich. Indeed, I never lived in Zurich again, after that. So in 1929 I had two possibilities: I could have gone to Born, who had an assistantship open for me in Gottingen; and Kramers also, who suggested, I think, to Pauli that he could get a fellowship for me in ... Utrecht. And I chose Utrecht — I think that was partly also Pauli's influence, maybe also partly Heisenberg's. Both were somewhat critical of the Gottingen spirit; that is, Born's school and approach were considered highly formal and mathematical. I think it was this feeling with which I was inoculated which made me feel —. Well, I mean I had been in Gottingen before, and I knew people there, but I had the feeling, 'No, I'd rather go to Holland.' Besides, it

may have been that I knew Germany already, and I wanted to go to Holland. So then in 1929 I came to Kramers, and, that was a very fine time because again he had a few students, but I think I was the first time that a Lorentz Fellow worked with him... And so he immediately accepted me as a friend, and. I was at his house, and he spoke to me about music and poems, of Oppenheimer, and things like that. But also physics, of course. And. I was very happy. The first thing I did at that time is that I caught this error in my thesis and wrote that paper. And then the second thing also: I found that at low temperatures Heisenberg's theory could be done better. Heisenberg had pulled some rather daring swindles there. I could show — you know this business — that when you go to very low temperatures, then almost all the spins are parallel —. I had essentially the idea of spin waves. And that gave an approach toward the treatment of ferromagnetism at low temperatures.

Kuhn:

I had meant to ask you earlier: In the published version of the thesis, the first of the conduction papers, you refer repeatedly to Hund's molecular

work. You constantly are drawing comparisons with what Hund has said about the two-center problem, for example, and the symmetric and antisymmetric solutions. To what extent was that also a part of your research?

Bloch:

Now, glad you reminded me of it. Of course, Hund was also in Leipzig. Although my contact with Hund was not as close as that [with] Debye, of course I knew him well. Now, here I don't really quite remember — I didn't even remember this particular point. But I suppose that it is true, that I started by saying, "Well, after all, one ought to generalize from two-center to many centers."

Kuhn:

One wouldn't necessarily do that. I mean this is an open question. There are to me a surprising number of references. One wouldn't have to take that approach, but one could.

Bloch:

I'm afraid I do not remember any more. I didn't even remember that I did refer to Hund's molecular orbits.

It's quite possible —. Well, there was of course also a little tendency at that time in a thesis to show one's erudition. And that's, for example, why I did bring in group theory. One sort of had to show, "I know this." [Chuckle] "I know something, at least, about molecular orbits." As you say, I mentioned it, so it must have played some role in my thinking. I don't remember this too well. I suppose that must somehow have occupied me at that time, this idea, knowing after all very well the work of Heitler and London, from Zurich. But that was one approach to dealing with molecules, and then there was Hund's approach. And I somehow probably felt, I guess rightly, that Hund's approach was the more suitable one.

Kuhn:

Was the more suitable one for your problem?

Bloch:

For the conduction. problem, yes.

Kuhn:

Were you caught at all in the opposition between these two approaches?

Bloch:

Not seriously.

Kuhn:

This bothered some people, I gather, quite a lot.

Bloch:

Sure, of course. Now, I mean again, I had some sleep-walking qualities at that time, because of course I did realize that it was an over-simplification that one treated the one-body problem and then had many-body problems. But I felt, 'Well, that's probably all right. It works, so let's not worry about how good this approach is.' Later on, when people kept on using this same formalism and kept on refining it more and more, I was simply amazed because I thought, 'Oh, well, that's of course only an approximate theory.' In fact, I was already rather amazed that the resistance law came out as well as it did. I thought, 'Oh well, interactions can play a dirty role any old time.' And so I had a sort of sleep-walking — I was not very critical, not towards myself, not towards other people. I just somehow felt, 'Well, these are the wave functions. They do the

job, so let's stick to them.' And that they actually weren't much —. Well, there were other people who were surprised too, that it turned out to be such a good approximation. Well, so I think that essentially covers it up to 1930. I did go back to Leipzig in 1931 — Wait a minute — '28, '29, yes — in 1930. In 1930 I was in Holland. I didn't spend all the time in Holland, by the way, with Kramers. I spent half a year with him, and then I was a few months or a few weeks — very nice — in Harlem. There was Fokker, of the Fokker-Planck equation. He was the director of a museum and laboratory there, Teyler's Stichting... When people go to Holland, they always go to the Frans Hals Museum. They should go to Teyler's Stichting, which is absolutely wonderful, both from the point of view of scientific instruments and art. And Fokker was the director. He wanted me as a sort of private tutor. He wanted to learn about Pauli matrices. He never had time, but (he) lived the life of a gentleman there. I had just a very nice time; I didn't really do very much. Then I went back to Leipzig in... the fall of '30. I stayed there until the summer of '31. There I was again interested in ferromagnetism, and I wrote a long and learned paper on ferromagnetism and hysteresis. I wrote a

paper with [G] Gentile, an Italian, on ["Anisotropie d. Magnetisierg. ferromagnet. Einkristalle"], all down-to-earth problems, an extended paper which later on became my Habilitationsschrift. That I wrote in the summer of '31. What other things I was interested in at that time I don't quite recall. I think Placzek was there, and maybe we talked about the Raman effect; I'm pretty sure we did. But nothing terribly profound.

Kuhn:

You had been away now since the very first year when you had been there before. There were many more students now. What did one feel the problems now were?

Bloch:

Teller was there then at that time. Teller worked with Hund on the ionized hydrogen molecule.

Kuhn:

Was the sense that physics had now solved its problems? Had one's sense of what there was for physicists to do changed drastically in this short

time? Or was Leipzig just bigger but much the same?FB

Bloch:

I have a feeling that among the younger people talk went around that quantum mechanics was getting dull, that one was just doing some more of the applications, and that the most exciting things were really done. About the difficulties which were looming in the background, yes, I believe there was some talk about the beta decay and the difficulties of keeping electrons in the nucleus. I'm not quite sure whether it was at that time. But I think that was one of the things —. Again, I don't believe that I was terribly excited about this. There were difficulties, yes, but I did feel it was particularly my job to solve them. I didn't see any way of solving them, and probably I did not quite realize the profundity of it. I think that was only Bohr who convinced me that it was absolutely impossible to imagine an electron to be present in the nucleus.

Kuhn:

But your earlier attitude, you think, would have been more like your attitude towards superconductivity,

that the model had eluded you so far, rather than that it was a point of fundamental importance?

Bloch:

A very good way of putting it, actually.

Kuhn:

Well now, you go from there to Bohr. How did you happen to do that?

Bloch:

I'd met Bohr before. I think it was Heisenberg's recommendation. Heisenberg thought that it would be good for me to spend some time in Copenhagen. Bohr invited me — I don't know whether that came via Heisenberg — and I could get an Orsted fellowship and go to Copenhagen, which I did at that time in a very devious way. I went via Russia; that was my first and last visit to Russia. I was invited by Landau. I went east and went to Kharkov, Moscow, Leningrad, and then came through Finland and Sweden. That's the way I came to Copenhagen. Then, well, it [Copenhagen] was a very strange thing. I tried to describe it somewhat in the *Physics Today*. Did you see that? Do you want a reprint of

that, by the way? It gives a sort of feeling of the —. With Bohr one didn't talk about things in any systematic way, but Bohr liked very much to sort of think out loud. Since I was essentially—I lived in the same house — he thought a lot out loud in my presence. He kept on going back to the indeterminacy relation, and what it all meant. He rather felt and gave me the impression that Heisenberg had rather oversimplified the problem. Really, it was at that time that I began to understand the whole problem of measurement, that one cannot show a sharp line of distinction between the observing subject and the object to be observed, and that this is really as profound as that, and that the whole principle of causality is at stake.

Kuhn:

Who talked about that in Copenhagen then except Bohr? By then I think nowhere except in Copenhagen were those problems still being discussed.

Bloch:

Of course, I was not everywhere, but I must say I know that in the places where I was, it was not

discussed. Kramers was not particularly keen about these either. No, it was for the first time in Copenhagen that I felt they were important problems.

Kuhn:

I have some feeling that even some of the people around Bohr were no longer much interested in these.

Bloch:

We all, and I myself included, felt, very unjustly, that Bohr rather exaggerated these things. We felt, 'Yes well, they are important, they are illustrations already.' But Bohr kept on saying, "If somebody doesn't shiver when he learns quantum mechanics, then he hasn't understood it." It's a marvelous, marvelous —. But this, we felt, was rather —. We said, "We are not such cowards . We don't shiver." We realized of course what Bohr meant. And this dawned on me only very, very slowly, later, how profound this thing goes, and that one really has to shiver. Bohr said many things. Have you heard this wonderful statement — he told that to me himself; he was rather proud of it — that he made to Planck?

Planck visited him early, maybe '26 or '27, and told him that he thought that quantum mechanics was so difficult to understand. Bohr said to him, "It isn't at all difficult to understand; it's impossible to understand." The causal approach that Planck wanted to bring into it. It was something that only gradually, I must say, and almost against my own wishes, became inculcated into me. It was Bohr's strong desire which —. Evidently he had a rather good opinion of me; he thought it was a shame that this man should run through life and be so blind and not even realize all the dangerous depths. He was rather insistent on that; and, as I say, I took it rather reluctantly because, as you may have gathered, I was more of the aggressive type and felt that solving problems was the interesting thing. Bohr said, yes, he was also interested, in problems, but this was the important thing. And he —

Kuhn:

He did this to you in walks together —?

Bloch:

In walks, or sitting —

Kuhn:

But it would be the two of you together usually?

Bloch:

Usually, usually. Sometimes it was also in seminars, which were very small. Muller was there. There was this poor man Solomon, Jacques Solomon, who was later shot by the Germans; the son-in-law of Langevin. He was there at that time. There were only a few of us. I think that I was not exceptional in that respect. Perhaps Muller, through longer acquaintance with Bohr, was more aware.

Nevertheless, this idea that the old man is life too hard was rather general; tong us. Bohr was very kind, and had, a good understanding (for young people). Yes, these things I think mostly, I remember, came in talks. I was not supposed to. He was always behind in his publications and had to give speeches, and I promised him that I'd write them up. Then I was his assistant; he would discuss with me or occasionally I would jot a sentence down, which immediately had to be crossed out again. In the meantime Bohr walked 'round and 'round and 'round. Then he always said, "Let's talk about something

else." And we talked about something else. So in bits and pieces, what I learned from him I got that way. He illustrated these things very beautifully. I don't think he invented that, but I do remember that it gave me a real jar — I really understood for the first time how profound things are — this famous double-slit experiment that you know. He published it later. That is to say, he said, "Really, you cannot possibly, you shouldn't be able to, tell whether the electron goes through one hole or the other, because, if you could tell, then you couldn't explain the facts." That gave me perhaps the first real shiver, but I said, "Yes, yes, it's true." I mean one has to abandon some very great prejudices.

Kuhn:

Has this had any real effect on your own later work?

Bloch:

Of course, as you know, I have never meddled myself with epistemology; it just doesn't suit my character. I do believe that it had some effect on it in the sense that I became more interested in the basic phenomena underlying some problem than just in producing results. I think it's rather clear in the paper

which I wrote in Copenhagen. There I took up an old subject of Bohr, again through his stimulus, this question of stopping power of particles. I would not have treated such a problem before, because I felt that the answers were already essentially there. In classical mechanics, by Bohr; in quantum mechanics, by Bethe. Through the discussions with Bohr I felt there was something basically unexplained. I mentioned it to you briefly. I would say that a paper like that, which I said is essentially of pedagogical interest, I don't think I would have written before. I'm thinking back now of it with very great pleasure, and I'm very glad. I did it. But that was Bohr's influence.

Kuhn:

Did the greater sensitivity to that sort of problem stay with you?

Bloch:

Yes, I think that stayed with me. I think from that time on, whatever problem I dealt with, I've always felt more like saying, "Let's go back to the beginning; let's see what the foundations are," rather than to produce results. Although I turned away

from engineering, I think I was too much of an engineer in regard to physics before I came to Bohr. Although I never became a philosopher, I think that besides producing results, basic understanding is an essential thing of physics really. It didn't become clear to me as essential until I met Bohr and talked to him about these things. There were many things I learned from Bohr. Not only about these but of course Bohr was a master in classical physics. And once in a while when he was in the mood, he was able to explain to you certain parts of classical physics in such an absolutely simple and marvelous way that I've never forgotten. In fact, you know, his own thesis was also on the conduction of electrons, so we had a common ground there. Once he told me, which I'd never known, in a very simple two lines you can derive, from a classical point of view, the expression for resistance. If you do the right sort of thing with the mean free path you get it in two lines. This is what Bohr was so great at. That is, he could get results within a factor of two or so. In order to understand the basic things you need not much mathematics that's what I learned from Bohr. Well, of course, this went on for a long time. Bohr made several statements at that time which I didn't

appreciate and some of them I don't even appreciate to this day, and I feel bad about it. Bohr used to say and kept on saying that the dilemma in quantum mechanics is this: that all observations are essentially classical. That is to say, he said that the only way we can make contact with reality is through classical experiments. And this is where all the difficulty arises, you see. This is not —. I don't think I want to go into it because in some sense it is clear; in another sense, why it should be quite as important as that —. I must say, it's only maybe in the last ten years or so that I have a certain feeling for that. But I'm not this type, you see. I've not gone deeply into that problem, and Bohr felt very sorry about it and probably felt it was one of my shortcomings. I think he felt one should go into these questions deeply.

Kuhn:

Of course he did. It's, I think, some part of the tragedy of his later life that no physicists really followed him in pursuing that concern.

Bloch:

Of course, one of the reasons was that probably we all felt, "There's no use going into that because Bohr is so far ahead of us anyhow that one can only play a second-hand role." I mean Rosenfeld, rather touchingly, did that. Rosenfeld was really just a help to Bohr. Bohr wanted somebody who knew mathematics and was willing to go through it; [Rosenfeld] sort of sacrificed himself. But it's true, I also have a feeling that —. Some of his remarks I remember; I'm sure many I have forgotten. If I did remember them and other people remembered them and would think more about them, we would probably see, well, in some cases he was wrong. For example, the question of beta decay. He was quite convinced that energy was not conserved. Well, all right. It was not that he was always right.

Kuhn:

No. That particular idea — that energy is not conserved — is one that he had over and over and over again in his career.

Bloch:

Yes, yes. He never felt that the conservation of energy is something so very, very sacred. That was, of course, more a formal argument, but Pauli —. The reason why Pauli didn't accept that was that Pauli, I think, appreciated more than Bohr, from a formal point of view, that "How could one do all these things if one didn't have a Hamiltonian?" And of course, the moment you have a Hamiltonian, you do have energy conservation. Pauli just thought, "Goodness! If we give up energy conservation, what a mess we will be in from a mathematical point of view;" Bohr was never impressed by such things, you know; he always felt, "The mathematicians will take care of that." I'm quite sure that it was this kind of thing that made Pauli invent the neutrino. He just felt, "I'd rather buy a new particle than make my life that complicated." Bohr did speak to me occasionally about the early days, and he gave me — in fact I have them still here — some of the reprints of the early years, 1916 and so on. He pointed out to me, and quite rightly, that he did not take his own model as seriously as many other people did. He was very proud of that. The strange thing was that Sommerfeld who, of course, at first was skeptical

towards Bohr's theory, later on took it much too seriously, or at least Bohr felt so. He felt really that quantum mechanics was the answer to what he was always looking for. About these things he spoke to me. But that of course you know.

Kuhn:

There are things that he said to you that I'd like to know but don't know, but in general we have been over much of that ground. There are parts of it we can just not know, partly because Bohr himself died too soon in our work with him. The whole period that Kramers could have filled in —

Bloch:

But Oscar Klein must have known a lot about it. Klein and Kramers were there at about the same time, think.

Kuhn:

No, Kramers was really two years earlier than Klein and was sufficiently further along early, so that there are key developments in the whole idea of the correspondence principle in this period before Klein

is involved, and even more before Klein is involved with real understanding of what is going on.

Bloch:

This was before the dispersion formula? Kramers never talked with me about these times. I'm not sure; I don't remember it.

Kuhn:

You go from Copenhagen in '31 back for '31-'32 again in Leipzig?

Bloch:

I came to Copenhagen in the fall of '31; I went back to Leipzig in the spring of '32. There I was habilitiert. (I became a) Privatdozent.

Kuhn:

Right, and then you're there '32-'33. This is just the period that the neutron, the positron, the Heisenberg theory — these and generally this whole question of quantum mechanics in the nucleus became very —.

Bloch:

There I have a very deliberate impression. One day — oh, it was in the late afternoon — Heisenberg came by to the house where I lived, which was not very far away from the Institute, and said, "Oh, I'm glad you are here. Would you come with me on a walk? I'd like to tell you something." And on that walk he said, "I just want to have your reaction. I just thought about nuclei." Now the neutron was discovered at that time.

Kuhn:

Do you remember getting this news? Was it a big shock to people?

Bloch:

Yes, oh yes. In fact I think Bohr told me that. You know, after I was in Copenhagen, I met Bohr frequently. On one occasion we met in Berlin, and I think he told me in Berlin about it, about the discovery of the neutron. Oh, yes, he was very excited; so were we all. This was maybe — oh, I'd say that was perhaps not even a year later. am pretty sure that it was in the ... early summer of '32 or so.

Heisenberg took me on a walk and told me about this idea. I was simply amazed. Maybe I said that I was surprised, but mostly I listened. I remember I thought, "How can Heisenberg know all these things (for so sure)?" He said, — and this I was willing to accept — "Of course, neutrons are —." This business of the electron — I've told you before about that difficulty (of the nucleus containing the electron.)" I said to Heisenberg [Heisenberg said to me], "This is fine, and everything will come into order. Neutrons have, of course, a spin, and their mass is equal to that of the proton, or practically equal. Then all these questions of statistics will get into order, right?" And so he said—

Kuhn:

He saw that immediately, that statistics would get straightened out?

Bloch:

He mentioned that so casually that he made it sound trivial. There, I must say, to me it didn't sound trivial at all. In fact, as I say, I was surprised at Heisenberg's —. But he went directly on. He just said, "Now, you see, this is rather interesting,

because why is it that the atomic weight is almost exactly twice the atomic number?" "Well," he said, "this is a symmetry. There must be a symmetry." And in fact he had already a sort of a rough mass formula—I think it was later called the Weizsacker mass formula—but I think it was more the Heisenberg mass formula, where he pointed out that there must be for symmetry reasons a minimum in the PZ diagram, and that it was shifting the other way because of the Coulomb forces, for the heavier ones. So he had all the essentials there. He was very excited about it, and I just didn't say very much. He really just wanted to see whether, by any chance, I could catch him on a gross mistake. I think I asked him occasionally, "Are you sure of that?" He said — well, Heisenberg was very cavalier in such things — he said, "Well, that's probably —" There was one more thing that Heisenberg mentioned at that time which amazed me, and there I was even more impressed. That is to say, he had the idea that the distinction — that the proton and the neutron were really two states of the same thing. He didn't use the word "nucleon," to be sure, but he had the idea of a quantum number. I think later on it was called the isotopic spin. But he said to me that they were

clearly two quantum states of the same particle. In an outline, I got the nuclear theory from Heisenberg on a walk, and he published it immediately afterwards. He was very quick in writing such things up. There was a little thing about —. Shortly afterwards I was then also somewhat interested in the neutron, and in fact I was beginning to get interested in nuclear magnetic moments. I had an idea which Heisenberg accepted. It was the wrong reason, but there was a little grain of truth in it. I must have accepted right away that the neutron has a spin, and then I knew that even nuclei — even atomic nuclei — did not have a magnetic moment. Of course, this is nonsense because it's because their spin's zero. But I had the idea that this is an exact compensation of the positive [magnetic moments of the protons] and the negative magnetic moments of the neutrons, because they are in equal numbers, you see. I suggested that to Heisenberg and said to him — I don't think I published it anywhere; no, I don't think I did — but I said to him, "I think the neutron ought to have a negative magnetic moment equal and opposite to that of the proton." Heisenberg liked that idea. You can see what a fantastic instinct this man had, because of course the conclusion wasn't so

wrong, but the reason was completely wrong. Then, of course, shortly afterwards — that was also before I left Leipzig — Stern talked at one of the colloquia about his measurements of the magnetic moments of the proton and the deuteron. Then we knew. I mean we knew indeed it had a negative [magnetic moment] but not exactly equal. Then things became clearer. But that was the only direct effect that Heisenberg's idea had, on me at that time — in connection with the magnetic moment. Somehow I've always spun the same thread in my whole life, as you see, because it was always magnetism. As soon as neutrons and protons were mentioned, I asked what were the magnetic properties. That was something which somehow suited my character.

Kuhn:

What about the positron, which comes in very much at this same time, but which is not so expected?

Bloch:

That I don't remember. I'm not quite sure whether I didn't learn about the positron only when I came here.

Kuhn:

That's late.

Bloch:

Well, it was '34.

Kuhn:

No, you must have had it in Rome; the positron is also '32. This really comes very little after the neutron.

Bloch:

Pair-production, yes. I don't know. In Rome there was one thing, since you asked me about quantized amplitudes. This is the sort of game I played in Rome. I had written another paper on the energy — it had to do with stopping power too — and there I had considered the question of the vibration of a gas sphere. I published that. That was later; it was in '32, I believe. When I was in Rome, I wanted to understand a little bit better how oscillations — plasma oscillations and oscillations of electron gases — could be explained in terms of the elementary mechanism. I thought at that time that the quantized

amplitudes might be a good handle to do it, because you had, so to say, waves. The quantized amplitudes had wave character. Well, it was sort of a vague idea. Interestingly enough, there was not much contact with Fermi. We had personal contact with him — played tennis and told each other jokes, but very rarely talked physics. Fermi didn't really like very much to discuss physics. But at one point I just felt I wanted to —.

Kuhn:

Elaborate that remark that he didn't like to —. Do you mean that he didn't like to talk theoretical physics?

Bloch:

He didn't like to solve other people's problems, or to be bothered by other people's problems. He wanted to do his own problems. If people came and tried to push him into a line of thinking in which he was not interested at that time, he didn't like that particularly. I mean he was not impolite, but he made it rather plain that he wasn't too much interested. And that time I sort of crashed his doors — that was the only time I did it — because I felt there was something

vaguely resembling it, and I started to tell him about the quantized amplitudes. Fermi said to me in Italian, "I don't understand a single word." "Non ci capisco una parola," he said. It is true. He did not understand quantized amplitudes at that time. He just simply never had studied it. I'm sure he did not —. The problem which I was worried about didn't interest him at all. But I think he made a mental note at that time, "Perhaps I ought to look at quantized amplitudes." And he wrote his neutrino paper a few months later. This was typical Fermi. You see, he didn't think, as I did indeed before too, that quantized amplitudes were of much use. When he'd explain things in the seminar or so, he always went the very pedestrian way — x_1 , x_2 , x_3 — he always wrote them all down. And then he thought, "Well, perhaps I ought to learn that." When Fermi learned something, he didn't just learn it, but he used it. Well, of course he realized the relation between quantized electron amplitudes and quantized field amplitudes, and the possibility of emission of electrons, of creation. I think actually I can really say that Fermi did not know much about quantized amplitudes in the fall of 1933.

Kuhn:

That is very interesting, because it then comes so quickly; and it's exactly at such a strategic time that he chooses to learn about them.

Bloch:

I'm quite sure that for him the beta decay was sort of a school example, one of the nice applications of quantized amplitudes.

Kuhn:

Very nice —

Bloch:

Very nice. The positive electron — Gosh, I wish I could remember when that came first to my attention. You say that was '32?

Kuhn:

Yes. I can't remember when, but '32. There, you see, my memory is not so very good. I have the impression that I heard about it later, but I must have heard about it earlier.

Kuhn:

I asked because the whole reaction to the neutron and to the positron was very different in some places. Bohr, for example — it was terribly hard to persuade Bohr that there was such a thing as the positron. me, how did you happen to come to Stanford? How you came to America think the time makes fairly clear. But how did it happen to be Stanford?

Bloch:

In detail I do not know. The thing was this — well of course, it was known that I had left Germany because of Hitler, together with several other people — Franck and Einstein and so forth. At that time the Rockefeller Foundation offered to various universities funds to take on these people. They would pay, I think, for a year or two, their salaries. They would do that providing that these institutions had at least a serious intention of keeping the people if they were suitable. Why it was Stanford —? I think there must have been several reasons. In the first place, since this work on fast-moving particles was of some interest to the people here who were

working in X-rays at that time, they knew about this work — [D. L.] Webster did, I think; and I think Oppenheimer has at one point indicated to me, that Oppenheimer had also something to do with it, because I knew him from Zurich before. I think this was the reason why I got an invitation to come to Stanford. That was when I was visiting in Copenhagen in '33. Needless to say, I accepted it. That's all. Actually ... I had a Rockefeller fellowship at that time. When I applied for [it], I said I wanted to spend half a year in Rome and half a year in Cambridge. Then I cancelled the Cambridge thing; I said I'd rather go to America. That's how I came here.

Interview Session 2

Weiner:

This is a tape-recorded interview with Professor Felix Bloch. We are sitting in his office in the Varian Physics Building at Stanford. We mentioned in our earlier discussion that in your interview with Tom Kuhn conducted in 1964, a good deal of your early background, your early work in Europe, your work on electron conduction generally, and your thesis work with Heisenberg had been covered. He had taken you through about 1933. There are a few general questions I would like to ask about that period. It seems to me that there was a great deal of mobility in your career, because you were able to hop around on one fellowship or another recommended by some of the leading physicists in Europe. And in the course of this you were exposed to different styles, different traditions of research. Now, on the surface it seems that this might be true—that they were different traditions—but in fact were there clear divisions?

Bloch:

No. You see, the physics of that time was very different from what it is now. There were only relatively few people, at least better known people. They were rather small in number. And it was really a family, and everybody knew everybody else; so therefore there was a great deal of unity. And although there were, of course, great individual differences, I think there was a common purpose all over Europe at that time. And I'm sure it's due to the fact that the number was so small that everybody knew everybody else personally—his family and so forth. It was a very close relationship, much closer than it is now with the great number of physicists.

Weiner:

Do you think this was due as well to the short distances between one center of research and another?

Bloch:

Well, of course, the distances were shorter than they are now, but, on the other hand, travel was not as simple as it is today. To go from Leipzig to

Copenhagen took a full day at least. But it was nothing that people at that time considered as prohibitive. There was a good deal of visiting all the time.

Weiner:

This was the normal course then of immediate post-doctoral education?

Bloch:

Well, if you refer to my having worked at different places, I would say yes. This was what one hoped and expected to do after one's doctorate—to work at different places and get in contact with the different leading physicists. I considered this perhaps the most important time of my education as a scientist—this period of traveling in Europe to the various centers before I came here.

Weiner:

What did you have in mind to do when this was over?

Bloch:

Well, that was a question which one did not really too seriously consider. The hope was that eventually one would associate oneself with a university and join one of the universities. It was by no means clear where that university would be or which it would be. Nor could I say that it really concerned us very much. I think I can truthfully say that we were really so engrossed in our work that the question of where it would lead to materially was, in a certain sense, a secondary question. Of course you do remember or perhaps you know that I established myself at the so-called habilitation as a privat-docent, which is the first stage of the academic career, in Leipzig in 1932. So already before I left Europe I had some kind of a university association, but that would by no means mean that I would know where or when I would get a professorship.

Weiner:

You had first gone into engineering, and apparently you had some doubts or other people had doubts about careers in physics itself. Now, by the time you got involved in theoretical work, obviously this was

not an issue anymore. You were doing physics and you were doing it happily.

Bloch:

That is correct, yes.

Weiner:

But did you have any general feeling about the opportunities to pursue this kind of work? That is, in regard to other countries—did you think the situation in Germany provided more opportunity or less?

Bloch:

Well, of course, it so happened that due to the presence of several of the leading physicists in Germany, Germany was probably, I would say—maybe next to England, though I'm not sure—the leading country in theoretical physics. Besides it was a large country with many universities, so a priori the chances of joining a university were probably greatest in Germany; but I cannot say that I had any fixed ideas about that. Switzerland, which is my home country, was certainly another possibility. Besides I must also say that the idea of eventually

going to America was by no means a remote or fearful idea at all. I had discussed that often with my friends, some of whom, as you may know, went already to America quite early. So this was also another possibility. But, as I say, it was not a matter of prime concern really at that time. We had the confidence somehow or other, rightly or wrongly, that sooner or later we would find a position all right, and we were really in no hurry. We were also very young.

Weiner:

Good things come to good physicists.

Bloch:

Well, at least we hoped so.

Weiner:

In your discussion with friends about the remote possibility of coming to America, what was raised as the advantage or disadvantage of doing such a thing?

Bloch:

Well, there was, of course, a certain spirit of adventure behind it in any event—to see new parts

of the world. I had this adventurous spirit quite a lot and several of my friends had it, too: There was also, of course, the fact that we knew that the possibilities, the openings, in America were more numerous than they were in Europe. And perhaps somewhere way back, although I can't say for sure, there was also perhaps the feeling (though I may be constructing things backwards) that it might not be a bad idea to go into a country where the tradition is not so highly developed and stand on one's own feet. I don't believe you should overestimate the importance of these remarks. It looks as if I had had prophetic foresight, which I did not have. I'm sure that the main idea was that, indeed, we were Europeans, and it seemed natural to stay in Europe; but, as I say, the idea of going to America was not a strange idea at all.

Weiner:

Well, you were at Leipzig; you had the beginnings of an academic career. But to get a professorship—although you weren't consciously worried about it at the time—you would have had to wait for some opening because there was generally not more than one theoretical physics professor at a university. But

since you were in a circle involving the leading people, you didn't worry too much about this.

Bloch:

No, not too much.

Weiner:

In '32 you'd gone back to Leipzig, I guess, starting in the fall-

Bloch:

No, in the spring of '32 I went back to Leipzig.

Weiner:

So then you were beginning on the academic career there. Now, what happened to change things? Let's go back a bit. Were you aware of the changing political situation?

Bloch:

Oh, yes, indeed. One could not help being aware of that. Of course, Nazism was growing in Europe. It had already been growing for several years. The University of Leipzig was a particularly good point of observation because the students of the University

were among the early enthusiasts for Hitler. So we noticed that very strongly. There was nothing violent about it, but it was quite clear that these ideas were gaining ground. It was quite obvious to me, in any event, that fearful changes were on the horizon.

Nevertheless, I was happy at that time. I had a very close relationship to Heisenberg, and I was happy to do my first teaching at the University of Leipzig. I don't quite remember now, but I think it was already in '32 that I applied, or maybe Heisenberg applied for me, for a Rockefeller fellowship; and it was very clear to me at that time that this might come in very handy in the sense that my staying in Germany would be limited. I'm talking about '32, especially about the fall of '32, when it didn't take great foresight to see that things were coming to an end.

Weiner:

You say that the students were enthusiasts and were taking up with the Nazis. How did this manifest itself? How did you become aware of it?

Bloch:

Well, I had contact with students in places where I lived. I rented a room or two in a house with other

students together and we talked about these things. There was nothing aggressive in their attitude at that time. But they felt that as an outsider, as a Swiss, as they considered me, I was entitled to some education; and they gave it freely to me. I did not hide the fact that I did not share their opinion, and they accepted that as one of the facts; but it was very clear that they were taken in by this. It, of course, was the time of the depression. And so the atmosphere was very awesome.

Weiner:

How about anti-Semitism? Was there evidence of this?

Bloch:

Well, it was a strange thing in Germany. You see, anti-Semitism was not a social phenomenon as it is in many other countries. It was almost more of a theoretical attitude. That is to say, I knew many Jews and Jewish families in Leipzig, and I don't hardly recall that they experienced violent anti-Semitism in the sense of beating or things like that, which occurred, as you may have known, in Hungary and Austria. That did not exist in Germany. But it was a

dogmatic philosophical anti-Semitism, which at that time existed side by side with otherwise perfectly normal relations.

Weiner:

How did it affect one's university career?

Bloch:

At that time it was just starting. I don't think that my own habilitation—of course thanks to Heisenberg's support and authority—made any serious difficulties. But the first dismissals of Jewish professors started already at that time.

Weiner:

In '33?

Bloch:

In '32 even. In '33 they were practically all dismissed. In '32 there was the case of Lessing and other people. The official anti-Semitism made itself felt.

Weiner:

I was thinking of the official laws that came out, I guess in March, indicating who could and who could not be retained and so on.

Bloch:

I'm talking about preceding that. Of course, as soon as Hitler came to power in March '33, then the picture changed abruptly, and dismissals occurred right and left.

Weiner:

Did Heisenberg have any reaction to this? Did he discuss this with you?

Bloch:

Yes, of course, we discussed it a great deal. Probably Heisenberg felt perhaps that my fears were somewhat exaggerated, but it was not that he considered them groundless.

Weiner:

Perhaps then, as you suggested, the application for the Rockefeller fellowship, which had to be initiated by him-

Bloch:

Yes. I think it was initiated by him—not quite sure.

Weiner:

Well, someone has to do this according to their rules.

Bloch:

Yes. He may have done that, and also Debye may have been in on it.

Weiner:

By the way, I can check that because the listing of the Fellows shows who did the recommending*, It could have been either of them— that's right; they were from the same institution. Why did you pick Rome? *The listing of the Fellows shows who did

the recommending only before October 1930, and does not cover Bloch's fellowship in 1933.

Bloch:

I've tried to remember the exact details. Well, I think it was partly again perhaps a spirit of adventure. I wanted to go to an environment quite different from the one I knew in the northern countries of Europe. I had met Fermi, of course, before and had great respect for him. I think it was also partly the cultural attraction: it was an interesting historical city. But I think probably the main reason was the presence of Fermi. I don't believe I would have gone to Rome without Fermi being there. You may recall that I split the Rockefeller fellowship into two parts—half of it to be spent in Rome and half of it to be spent in England in Cambridge.

Weiner:

But you didn't consummate the second half.

Bloch:

No, I went to this country.

Weiner:

Well, the reason I brought up the fellowship now is because we were talking about 1932, and that's when you made application apparently. When did it become clear to you that you no longer could remain at Leipzig? Were you dismissed?

Bloch:

No, I was not dismissed. I simply quit. I went home to my parents in Switzerland. I received, in fact, later, a letter from the dean of the University of Leipzig begging me to come back and teach my course, which I had announced, and rather ironically promising that I could get guards in my lecture room to prevent any troubles which might arise. That was rather ironical, because the only people who would have made trouble were the guards, because the few people who sat in my lectures were friends and they certainly wouldn't have made any trouble. I didn't even answer that letter.

Weiner:

When was all of this—in the spring?

Bloch:

Yes, that was in the spring of '33. You see, I left Leipzig in March of '33 and never went back, but this letter may have come—I don't know—in April or May. I'm sorry to say that Heisenberg, rather naively at that time, also felt that I should certainly return. He saw no particular reason why I couldn't go back. And I may say that from the spring until the fall of '33 I had no job. My Rockefeller fellowship started then, and so Heisenberg evidently felt: "Well, if he's free anyhow, why can he not at least give us a lecture while he is here?" I mean my very strong feelings about what was going on in Germany he apparently did not quite understand.

Weiner:

And so you actually physically left just after the Nazi laws came out at the beginning of March and you went home.

Bloch:

Right.

Weiner:

There's something interesting here, because according to the rules of the Rockefeller fellowship, if I remember correctly, you had to have a permanent position, a position to which you could return before you could go on their fellowship. I may be confusing it. There were two sources of funds. One was the Rockefeller Foundation and one was the International Education Board. It's really the same thing, but the rules were somewhat different.

Bloch:

It was in fact the International Education Board. Outside of America it was not called Rockefeller. It was called International Education Board.

Weiner:

But what you're saying is that perhaps because the application was made in 1932-

Bloch:

At the time that I made the application, I certainly had a position as Assistant and Privat-Docent both at the University of Leipzig. Now whether they

afterwards considered this fact to be very serious or not, I do not know. As I say, I never officially resigned, nor did I get a letter of dismissal from the University of Leipzig. I simply left and that was it. But the people at the International Education Board undoubtedly—in fact, I'm quite sure—knew what was happening, and if they had such rules, I suppose they simply disregarded them. I think it was probably true that from a sheer legal point of view I could have gone back to Leipzig. After all, I was not a German. As I say, I never went. I'm sure nobody at the International Education Board expected me to go back to Leipzig.

Weiner:

Who were you in touch with during this nearly six-month period before you went to Rome?

Bloch:

I spent part of it in Zurich, and of course in Zurich there were Pauli and Wentzel. Zurich was not a minor part of physics itself. In fact, I did some work there on quantum mechanics; as a matter of fact, I wrote a paper there during the time I was in Zurich. Then I was invited, to my very great pleasure, to

give lectures at the Institut Poincare in Paris, where I spent two or three weeks. It was a very wonderful time. I stayed at the house of Langevin. I knew his daughter and his son-in-law quite well. I cannot say that I was at all unhappy, nor did I lack occupation during that summer. Then after that I went to Holland to visit Kramers again, with whom I had been before. And there again it was a fruitful period. We discussed physics and various ideas. And then I knew anyway that I was going to go to Rome in the fall, so the summer went by very interestingly. Of course there was also a great deal of political discussions about what was happening in Germany at that time.

Weiner:

Was there much feeling that this would blow over; that it couldn't last?

Bloch:

Well, I don't think too many people outside of Germany believed in the thousand years of Goebbels. But certainly I did not think that it was something that was going to disappear soon.

Weiner:

How about others in Europe in that position? Were you in touch with them during this period?

Bloch:

Physicists?

Weiner:

Yes, other physicists who were either dismissed or resigned or felt that their future in the German university system was limited. Of course you weren't in Germany?

Bloch:

I was not in Germany during that time? I don't recall whether any one of the German physicists visited at that time. Whether I saw them even in Zurich, I don't recall at this moment, though I remember at one point I saw Weyl. Weyl came to Zurich during the summer of '33 and told us about all that was happening in Germany. But of course we knew it anyway.

Weiner:

It's very interesting: Von Neumann was in Germany visiting: He spent half his time in Princeton and half his time in Europe in '33, and he wrote a letter on June 19th. He had just been through Berlin and through Gottingen, and he was writing it from Budapest. He told of the situation there, and then he gave a list of people who were in need of assistance, who were dismissed or who soon would be? And he used the term, "beurlaubungen," forced to leave, and your name was on the list. There were others—all of them prominent theoretical physicists. He listed them. He said, "Here's a list of leading theoretical physicists whose future in German universities will be impossible?" So apparently others knew of your plight, and he was sending this letter to let it be known so that in case positions came up—

Bloch:

Do you know to whom he directed that letter?

Weiner:

Yes, to Oswald Veblen, a Princeton mathematician.

Bloch:

I see, because when I received this offer to Stanford, I realized that it was known and in fact there was a list of scholars and I do not know if it was the same. Von Neumann circulated it at that time, and my name was on that list.

Weiner:

This list was from the Academic Assistance Council in England, was sent over to this country to an American group called the Emergency Committee in Aid of Displaced Foreign Scholars.

Bloch:

I see. That's very interesting. I didn't know that.

Weiner:

In two weeks I'll be working with their files in England. I can check it out and see if your name is on the list. [Bloch's name is on the November 1934, Academic Assistance Council list of "Displaced German Scholars Available for Academic Positions?"]

Bloch:

Well, I was often really wondering to whom I'm indebted for having received this offer to Stanford very early, because it was undoubtedly on the basis of this being known.

Weiner:

The way it had to be done according to the regulations was that the offer had to be made by the university, by the department; and then they could apply for funds to one or another of the groups to consummate the appointment. The point is that it wasn't a question of just the placement bureau as such.

Bloch:

No. They got the salary for the first two years.

Weiner:

Right. It had to be initiated locally.

Bloch:

Yes. But nevertheless the fact that they knew that I was away, that I must owe to somebody, I've

sometimes thought it might be Oppenheimer, who knew me also and was at Berkeley. They had received my name from someplace. I thought maybe it was this list that was circulated.

Weiner:

It could very well have been. They were widely circulated. The reason I ask that is that in this interim period you knew you were going to Rome, and I would think that then you would wonder what would happen after Rome, or after Cambridge which would have been the other half of your fellowship.

Bloch:

Yes. Well, again, as I recall it, I was not too greatly concerned about that. I should say that I think it was still in the fall of '33 before I went to Rome that I received the offer to come to Stanford, and I must say that I was very happy about that. So it was certainly true that I would have to go someplace. I felt maybe at that time if I went to England, perhaps I could stay in England—at least for a while. But when the offer from Stanford came, and after some thinking, it was clear to me that that was what I should do. But I don't want to give any false

impression by any chance that I was, for example, looking frantically for a job at that time. I was young, and for the next year I knew what was going to happen to me, and one did not look too much into the future.

Weiner:

Well, according to something—again maybe it's what you told Kuhn in the interview—you got the specific offer for Stanford when you were in Copenhagen.

Bloch:

Yes, that's right.

Weiner:

Now, was this after the first six months at Rome that you visited Copenhagen?

Bloch:

No, this was before I went to Rome. As I recall it may have been in August or September. Then I went to Rome.

Weiner:

Well, that's another stop that you didn't mention. You mentioned Paris and Holland.

Bloch:

And I also went to Copenhagen. That's right.

Weiner:

And what were you doing there that time?

Bloch:

Well, I often visited Copenhagen after I'd been there before in '31 and '32. I visited occasionally, and that was just one of the visits. I was completely free that summer, and so I went there to see my friends and see Niels Bohr. It just happened by the sheerest coincidence that the telegram arrived while I was there. I think the people here [at Stanford] probably didn't know that I had left Copenhagen and thought I was still there.

Weiner:

What was your reaction when you got the telegram? Was there any other thing before that? Was there a letter or anything feeling you out?

Bloch:

No, not at all. It was just a telegram signed by Webster, chairman of the department; and I must confess to my great shame that I didn't even know where Stanford was, and only the fact that the salary was mentioned in dollars made me suspect that it probably was in the United States. There's a rather amusing story there. Heisenberg was also in Copenhagen at that time, and I went to him and asked him. I knew he had been around the world, so I asked him whether he knew something about Stanford, and he said he only remembered it vaguely. He said, "It's somewhere on the west coast and nearby is another university, the name of which I've forgotten," and he told me, "They steal each other's axe." Now, you may not appreciate this, but this was a sort of a game with students. Before the big football game, Stanford has a symbol, an Indian axe, and the Berkeley team stole that. This incident

was the only thing that Heisenberg remembered about Stanford. Also the name of Webster, I'm ashamed to say, didn't mean anything, either to me or to him.

But then I went to Niels Bohr, and Niels Bohr did indeed know the place, and he advised me. He said, "It's a very fine place." He advised me strongly to accept it.

Weiner:

Bohr was active at that time in trying to find what the situation was in Europe, sort of taking an inventory, of who was in need of jobs. He was one of the people who contributed names.

Bloch:

In this case it was most direct. Because I was in Copenhagen, I went with the telegram and showed it to him and said, "What do you think?" And then he told me about Stanford and said, "If I were you, I would take it." I not only took it, but I wrote to the Education Board and said, could I please go to Stanford in the spring of '34 rather than going the end of June. The offer for Stanford was, in fact, for

the same fall of '33, but I did want to take my fellowship at least in Rome.

Weiner:

Did they approve that? In other words, did you come over here for the second semester of your fellowship as a fellow?

Bloch:

No. When I came here I simply relinquished the fellowship, with their approval.

Weiner:

You had to notify them.

Bloch:

oh, yes.

Weiner:

You mentioned that you had had some friends who came over earlier. Who were they?

Bloch:

Well, there was Wigner and Von Neumann particularly.

Weiner:

And you had known them in Europe.

Bloch:

Yes. But then I had also heard about America from Ehrenfest. He came to Ann Arbor quite often, and I knew him, too, and he was very enthusiastic about America; so I had heard about the American scene before from the Europeans. Also I had met, of course, American physicists in Europe before I came here.

Weiner:

Who were they?

Bloch:

Van Vleck, particularly, and Houston.

Weiner:

That is, people in your field.

Bloch:

Yes, of course. I met them as colleagues. Van Vleck I think I met in Holland first and Houston I believe in Leipzig. He was at that time with Sommerfeld. There were some others, too, but I think these are the most important.

Weiner:

Yes, Van Vleck went to that Solvay conference in 1930—the one on magnetism.

Bloch:

Yes, but I don't believe I met him at the Solvay Congress. I met him in Holland on one of his visits. He came to Holland quite frequently.

Weiner:

When Bohr told you about Stanford and you began putting an image together of the place, what physics did you associate with the institution?

Bloch:

Well, I must confess again to my shame that I did not know too much what was being done here. I

learned, of course, later when I came here that they did very fine work in X-Ray physics, but I did not know that at that time. However, I must also say that I had met Oppenheimer before, and I knew that Oppenheimer was in Berkeley, and so therefore I knew that there would be at least some theoretical physicist with whom I could discuss my work. Although I did realize that I was going rather far away from the centers, the fact that I was not all by myself, that there was a fellow theorist in the neighborhood, certainly also had its attraction. But otherwise I must confess that if you ask me whether I imagined Stanford the way it was, I must frankly say no; I had no way of knowing what I would find.

Weiner:

You were adventurous.

Bloch:

Yes, I was adventurous.

Weiner:

And you came on Bohr's advice, too. Do you think that was the most important factor, that Bohr sort of verified that this was a reasonably good institution?

Bloch:

Well, I would say that at least unless I had had some assurance that it was a reasonable institution, I might have hesitated, although the idea itself of going to America—and particularly to California—was very appealing to me. But, yes, I would say that Bohr's recommendation, when he told me that this is a good place, decided me.

Weiner:

When you went to Fermi in Rome—Fermi who had had experience here—did you have any opportunity to discuss your future in this country?

Bloch:

I don't believe we talked much about that. Fermi, of course, knew that I was going to America and I just accepted that as a fact. I talked with Emilio Segre about it at that time; and he sensed already that things in Italy were not going to be too good either. In a certain sense he envied me for going to America.

Weiner:

Although at that time he had no particular plans.

Bloch:

No. He himself had no plans. That something that one finds perhaps difficult to understand nowadays, but of course at that time the inertia—the feeling that after all Segre himself felt—he belongs to Italy, and the feeling that people belonged to the country where they came from—was still quite strong in spite of everything.

Weiner:

I would understand that for Italy and for France and perhaps for England. Would you say this would be true for Germany?

Bloch:

Well, I would say of course it was made so absolutely clear particularly to the Jews who left Germany that they were not wanted there anymore, that they could not very well have any serious thoughts of coming back. On the other hand, I know very well that many of them, to put it very mildly,

deeply regretted the fact that they had to leave Germany. They felt that there were their roots and that they were torn out of them, and I suppose some of them hoped that they would return. They hoped that this would blow over and that they would return at perhaps not too late a time. Now, I must say that for me Germany was out and I think I can safely say forever at that time. I would not have considered going back. But of course it was a different matter because I was not raised in Germany.

Weiner:

Well, then in Italy you worked. You described some of this work in the earlier interview when you told particularly of how your discussion with Fermi may have started him on the path to the neutrino.

Bloch:

It's possible. But that sounds much too good really. It was more a technicality. We talked at that time about what was called second quantization. I had just begun to understand it. It actually went back originally to Wigner and Jordan, I believe. But it was considered rather highbrow at that time. I made some application. I wrote a paper in Rome and made

use of that. And I think it was more that Fermi, hearing me talk about this sort of thing and probably having also heard other people talk about it, felt perhaps that it might be time to get acquainted with it. I think the neutrino was almost an exercise for Fermi. Once he understood that he realized that the theory of the neutrino as he proposed it really essentially required the second quantization. So I don't want to say, by any means, that I suggested to Fermi the theory of the neutrino. It was only that I mentioned to him a certain tool that he was clearly not acquainted with at that time. He was rather reluctant at that time. He felt that it was probably a lot of highbrow nonsense, which one didn't need. But then in typical Fermi fashion, without saying anything, he went home and studied it.

Weiner:

And then once he knew, he might as well use it.

Bloch:

That's it exactly. Fermi, of course, once he understood something he really understood it. There was no such thing as half understanding.

Weiner:

Before we get you to the United States, I wanted to get back to this general business of having been in contact with Pauli, Heisenberg, Bohr and in a different sense with Kramers and then in the more recent period with Fermi-

Bloch:

Not in a different sense with Kramers. I spent half a year before that in Utrecht and worked very closely with him.

Weiner:

Kramers and then Fermi. You described how Bohr had given you a taste for something that really was not natural for you and that is the epistemological approach, which still isn't your own approach but helped you clarify your thinking on certain questions. It's very hard to identify elements of style and where these elements come from. But did you pick up anything else, do you think, that you can identify as clearly as that?

Bloch:

I don't quite understand your question. Do you mean in the sense of epistemology?

Weiner:

No. From Bohr you picked up a particular approach. Can you identify what you may have gained from Fermi or from Pauli and so forth? Now, the answer may be "no," that you can't identify it because it's intangible.

Bloch:

It is largely intangible, although on some questions, of course, it was quite direct. It was not that they told certain problems to me. Of course, I heard them talk about various things. I think I followed pretty much my own interests, which were at that time still in solid-state physics. But, of course, whenever I felt that there was something worth discussing I had discussions with these people, and very often of course got very valuable and important criticisms or even some suggestions about which way to work on them. These are things one cannot say. Even though they are not so easy to describe in exact words, the

influence and the contact with these people was of enormous importance. Then, as you say, their style of thinking: the fact that one can approach physics from very different angles was a great revelation to me.

Weiner:

It makes one tolerant.

Bloch:

It's not so much a question of tolerance as a question of widening one's horizons. Especially as a young man and having worked in a very special field and being primarily subjected only to a very constant and very important contact with one physicist, namely Heisenberg. Nevertheless I realized as I went around that Heisenberg's particular style, if you want to call it that, was not the only one.

Weiner:

Well, all of this is at the age of 29 with all of this background. And then you did come to the United States after Rome? Did you leave directly from Rome?

Bloch:

I wasn't even 29: I was 28 when I came here.

Weiner:

It seems that we're talking about a whole life story and yet all of that was within this brief period of time.

Bloch:

Yes. These years between 1927, when I went to Leipzig, and '33. When I left Europe, were probably the most important ones in my entire development.

Weiner:

With that you came here. Did you go directly from Rome?

Bloch:

Yes. Well, I think I stopped once more and visited my parents in Zurich, but then I came directly to the United States.

Weiner:

I assume you docked in New York. Did you proceed directly to the west coast?

Bloch:

No, I think I spent a day or two in New York. Breit, whom I had known in Zurich before, was very kind and met me at the dock and showed me a bit of New York at that time. But, as far as I remember, I spent only a day or two in New York. I remember especially warm feelings toward Breit. When you arrive at the shores of a foreign country and you meet somebody whom you know and is friendly, this makes a great deal of difference.

Weiner:

Knowing his love for walking, I wonder if you walked all of Manhattan.

Bloch:

I also remember that I still did not feel very good because the boat had been shaking and I had this peculiar feeling of land sickness. I was rather

depressed during those two days in New York, I remember, but that was partly fatigue.

Weiner:

Was it mostly social with him—I mean seeing the city? You didn't visit NYU or Columbia?

Bloch:

Certainly not Columbia. I don't quite recall at that point whether I visited his lab, whether he showed it to me or not. I certainly saw it at a later stage, but I'm not quite sure whether it was at that occasion. I think that it was not more than two days that I spent in New York.

Weiner:

And then you took the cross-country train?

Bloch:

That's right.

Weiner:

Directly to California?

Bloch:

Yes.

Weiner:

I can't help wanting to know what your impression was at seeing this country, 3000 miles worth, from a train window.

Bloch:

Well, of course, the enormity of it, especially at that time when you went by train, was I suppose the most impressive thing for me. Otherwise you don't see too much from a window of a train. But these endless plains and endless mountains and so on. I was not used to such large scales coming from Europe.

Weiner:

How about your command of English?

Bloch:

Oh, I think it was quite acceptable. In fact, I don't think it has improved that much since I am here. Once I knew enough English to get around, I don't think I improved—I learned English, of course, in

school and had spoken it before. So that was not a difficulty.

Weiner:

And when you got here, whom did you check in with? Who took charge of you?

Bloch:

Well, I'm sure it was Dr. Webster, who was chairman of the department. I don't quite recall, but I believe he met me at the train station. And then the first days are simply lost in a fog in my memory. I suppose I was so busy absorbing these impressions one after the other that I don't have an orderly, clear recollection of my first few days. All I can say is that people were exceedingly friendly here. I had a wonderful feeling of being really wanted, which after what had happened in Europe was not a minor aspect. Obviously they were clearly glad to have me here, so that was a very fine start.

Weiner:

This was in the summer or the fall?

Bloch:

The spring of '34. I think it was early April. I started to teach immediately.

Weiner:

That's right. You explained that before. What kind of teaching responsibility did they give you at first?

Bloch:

Well, I was not told in any dictatorial way what I should do, but it was clear that I was to teach the regular theoretical courses, which I proceeded to do from then on. I was free to choose what I wanted. I think I taught over the whole gamut. There were very few students here, and so I taught one course after another—electrodynamics, mechanics, thermodynamics, quantum theory—everything.

Weiner:

What level was this?

Bloch:

I think it was probably mostly graduate. I didn't teach any other courses. I had a very small audience.

Weiner:

And who else then was doing this? Was anyone else doing this work?

Bloch:

Well, Oppenheimer was doing it in Berkeley, of course.

Weiner:

But I mean here.

Bloch:

Nobody else.

Weiner:

Were there any theoretical physicists on the staff?

Bloch:

No, I don't think so. I was the first here: Of course, I must say that Webster, particularly Webster, knew a great deal of theory, amazingly so according to my feeling compared to an experimentalist in Europe. The distinction, the division, between experimental and theoretical physics was much sharper than it was

here. But it may have been, of course, partly because the professional theoretical physicist did not exist. I think Oppenheimer was the very first in California altogether. I don't think that existed before.

Weiner:

There was Tolman.

Bloch:

Well, Tolman also was an experimentalist. I don't think he was the pure theoretical physicist as it was bred in Europe and then came here. I think first was Oppenheimer, and I was probably the second here in California. So, therefore, the experimentalists here, by need or by desire, indeed were theoretically much better equipped than the experimentalists in Europe. This I found out. Nevertheless, of course, the refinements were not known here—particularly in quantum mechanics, which was really a new science at that time. Except in its rudiments, it was not known here. People knew the principle of it. I was the one who was to preach the gospel.

Weiner:

At Stanford, you mean.

Bloch:

At Stanford.

Weiner:

Certainly there were Van Vleck and Slater and others in the East.

Bloch:

I'm talking about Stanford. And I'm sure in Berkeley it was Oppenheimer, and then Oppenheimer went occasionally to Caltech. He more or less commuted.

Weiner:

This question of the lack of distinction between theoretical and experimental outlook within one physicist in this country as compared to the sharper divisions in Europe—was this a consequence of the training of physicists in Europe? Did an experimentalist get much theory in his work?

Bloch:

Well, he could get it. Of course, the whole system of education in Europe was much freer than it was here. You entered the University as a student and took any

courses you damn pleased, or you didn't take them—nobody cared. And, of course, the experimentalists had the opportunity—and some of them availed themselves of it—of taking theoretical courses. But it was perfectly clear to anybody who aimed at being an experimentalist that that was something that he should not spend too much time on. He had to get into experimental work if he was to be an experimental physicist. On the other hand, the theorist sometimes never went to a laboratory.

Weiner:

He didn't have to. In other words, it was not required. That's what I'm trying to get.

Bloch:

Nothing was required. There were no requirements at all ever.

Weiner:

It would be interesting to compare and really do a good study of what was required in this country in the education of a physicist, because if he's forced to take that much theory, we know at least he's getting

some background. It depends, of course, on who's teaching it.

Bloch:

Well, I mean the fact that there was no enforcement, I'm sure, helped this strong division. As you say, nobody forced him to do that. So therefore, once you decided, "I'm only interested in theory," well, that's what you did and simply didn't care about anything else. I remember it came in my case quite early in my student days. Quite abruptly at some point I simply decided: "No, I'm more interested in theory than experiment," and then I dropped experiment cold.

Weiner:

I guess it depends also on what stage physics is in, because there are some stages in the development of theory where you desperately need to be in very close touch with experiment, and there are other stages when it's not so important.

Bloch:

Well, of course, this sharp distinction, which I think was particularly outspoken in Germany, was not of

such a very old date either. I would say that it started around 1900 at the earliest, because the people before that—Hertz, Helmholtz, people like that—certainly did both. I'm not even sure whether Planck did not still do experiments as a young man. He probably did. Einstein, of course, was among the first absolutely pure theorists. I would say that this tradition of sharp separation was not that old in Europe either. As you say, it does depend upon the development, and this is very much conditioned by regional conditions.

Weiner:

It also depends on the personalities of individuals at a specific institution. At Gottingen where you had Born and Franck together, then you'd expect there to be collaboration, because the two of them were interested in it.

Bloch:

Oh, collaboration existed. That did not mean that the experimentalists and theorists do not talk to one another. Oh, they did that. But each felt that he had one technique and the other had the other technique.

Weiner:

But in some institutions there was less talking than in others. Let's put it that way. At Munich there would be less than at Gottingen:

Bloch:

That is quite true, yes.

Weiner:

But as the work developed here—you said you went into teaching— how did that compare with your teaching experiences in Europe?

Bloch:

Well, it was quite different because in Europe I had only taught two courses, two semester courses in Leipzig. I started in the spring of '32 and left in the spring of '33. And these were highly specialized courses. I remember I taught one course in general relativity. So these were entirely specialized. The regular larger courses were given by Heisenberg. That is, in Germany the senior professors taught the more elementary courses, and the special courses were left to the young professor with an audience of

two or three people or something like that. Well, when I came here that was a different story. Then I really had to teach what I considered the basis of theoretical physics. Besides, hardly any of my students had any intention of becoming theorists. I doubt that any became theoretical physicists. That was just part of their general training.

Weiner:

The ones here.

Bloch:

Yes.

Weiner:

It would be very hard then for you to compare preparation with the preparation of your students in Germany, since you were really teaching different kinds of courses.

Bloch:

Oh, entirely. I had a different caliber of students. As I say, in Germany in my very specialized courses I had only a few very advanced students, whereas here I had younger students and not very well trained?

Weiner:

What about the style of teaching? Were there significant differences in that?

Bloch:

Well, generally speaking, there was a great deal of difference. That is to say, the formality of teaching was far greater in Europe than it was here. Now, in my own special case the difference was not too great, because, as I said, the courses which I taught there were also taught before a small audience, and so it was more a matter of conversation; and that's how I conducted my courses here first, too. I knew all the students. I called them all by their first name, and the moment they didn't understand something they would ask and we would have discussion. But one can say, generally speaking, that I felt also that the larger courses were much less formal here than they were in Europe. There was much more close association between the teacher and the students than in Europe where the professor was on one plane and the student was on another plane, and very little did they meet.

Weiner:

I'd like to get back to teaching later. I was just asking now about first impressions. When did you first get in touch with other physicists beyond Stanford?

Bloch:

You mean here in America?

Weiner:

Yes.

Bloch:

Oh, I think quite soon. Well, of course, Oppenheimer I knew already, and we saw each other constantly.

Weiner:

Did you go to Berkeley?

Bloch:

Yes, we had joint seminars. Either I went to Berkeley or they came to Stanford, so we were very close at that time. Then I believe Oppenheimer took

me for the first time (I don't know whether I went with him or alone) down to Caltech, and I met other people there. I think I met Millikan for the first time there. So these were my first contacts in California. And, again, I don't quite remember when I went to the first meetings in the East. It wasn't the same year but I'm sure it wasn't later than '35 or '36 that I met most of the then theoretical physicists and many of the experimental physicists of this country: Again, there were not very many. At a meeting of the American Physical Society there may have been 40, 50 people, of which I probably knew one-third already and the second third of them I knew very soon.

Weiner:

The first one you attended was the Washington meeting?

Bloch:

I don't remember, but I believe it was at the Bureau of Standards in Washington. I believe so.

Weiner:

When did the seminars get started—the colloquia, the meetings with Oppenheimer and his students? How long after you arrived did you get in contact with them?

Bloch:

Oh, very soon. You see, Oppenheimer was the only person on the west coast whom I knew from before. I don't remember. I'm almost sure it was not more than a few days after I arrived that I went either to Berkeley or that he came to see me here; so that was very direct.

Weiner:

I'd like the full story, if you recall, of the seminars themselves. I've heard so many other people mention it as being a feature of the scientific environment here, the joint discussions and the fact that they got pretty lively at times, and they often had debates between you and Oppenheimer.

Bloch:

Yes. Oh, yes.

Weiner:

On one evening from quantum electrodynamics to—
I mean loads of things were mixed in.

Bloch:

Yes, yes.

Weiner:

I'd start at the logical point of how the idea came
about for such joint seminars and then what form it
took.

Bloch:

I don't remember. It was probably Oppenheimer who
suggested that, but it was a very natural thing.

Again, these seminars—there was nothing formal
about it. Sometimes we met here or in Berkeley and
sometimes we met someplace else—down in Carmel
or anywhere. The attendance was half a dozen
people, all of whom were known. So you shouldn't
think that these seminars were in any way
formalized. Whether we met actually regularly a day
a week, I don't know, but I think more or less they
were about every week. We ran them the way a

seminar is run nowadays. One of us would go up and tell about something he had thought about and read about, and then there would be discussions. It was very stimulating for me. I did not feel quite as isolated as I would have felt otherwise.

Weiner:

When it was in Berkeley, did your students come along, some of them?

Bloch:

I believe some of them did, although, as say, there was none who was particularly interested in theory.

Weiner:

The basic group would have been the Berkeley group with you as an addition?

Bloch:

Practically, yes. Besides Oppenheimer, there were not many: I think Melba Phillips was there at that time, a few of the old-timers, you know.

Weiner:

Was Serber there?

Bloch:

Serber was not there yet, no. We're talking now about '34:

Weiner:

Already in '34. I see.

Bloch:

Oh, they started right away. We had our meetings already in the summer of '34. Then, of course, later on, the seminar grew more and more.

Weiner:

But at first there was this small nucleus...

Bloch:

Oh, it was quite informal and quite private.

Weiner:

No money involved? No funds required?

Bloch:

No, of course not. We paid our own gasoline and that's all. More than that you didn't need. In fact, gasoline was cheap, too, at that time.

Weiner:

What seemed to be the recurring theme? It's true you take up something that interests you, and he does, too, and others in the group may-

Bloch:

Well, I don't think we even particularly collaborated at that time. I remember at one point I gave a seminar in which I reported about the then new theory of superconductivity of Fritz London. Well, it happened that this was something that hadn't been thought about before, and I told it to the people there. I don't know how much impression it made. It was just this sort of thing, that whenever you thought you had understood something, you told it to the others and they would ask you questions. I don't quite remember what Oppenheimer did at that time. I think he was already interested in the theory of showers. His interest was definitely in the direction

of cosmic rays and high-energy phenomena. I think that that's what he worked on. Now I knew about it, but I did not work in that field at all.

Weiner:

But you would participate.

Bloch:

Oh, yes, of course.

Weiner:

In the discussion.

Bloch:

Yes.

Weiner:

It was a theoretical seminar, though. Were there any experimentalists involved?

Bloch:

No, not in that particular instance. That was pure theory.

Weiner:

I know that at Berkeley they had a Monday evening journal club, which did bring together experimentalists and the theoreticians. Did you have anything of that type here?

Bloch:

Yes. We had a weekly journal club here too. But that was, I would say, on a much more elementary level. This seminar with Oppenheimer was really a high-level discussion and quite technical. Here the colloquium was aimed at a broader group. It was called Journal Club, as it is now.

Weiner:

Was there any parallel to that in Europe?

Bloch:

Oh, yes. Colloquia in Europe was very important.

Weiner:

I mean in the same sense as the Journal Club?

Bloch:

Well, I don't really quite know what the difference between Journal Club and colloquium is. Journal Club meant, of course, that people should tell what they have read, what they read in the journals and tell it to others; but this of course happened often in Europe, too. That is, if somebody talked to a colloquium, he would either talk about his own work or he would talk about what he read, and I think the same was true here.

Weiner:

I see. Because sometimes it's confusing: you think there's a whole new social institution being developed when in fact it's the same thing with a different name.

Bloch:

It's the same thing: We renamed our Journal Club, "Colloquium" some ten or 15 years ago. We somehow felt it was more appropriate.

Weiner:

The seminar started off with just a handful of people and grew, you said, in later years. Apparently you mean up until the time of the war. About how large did it get?

Bloch:

Oh, I would say the last seminars had at least 20 people. This was still not very large, not by today's standards. But I would say there were probably about 20 people. I mean at the early seminars it was a matter of course that after a seminar we would go to a restaurant and eat together. One would just ask for a not-too-small table and that was it. Later on when Oppenheimer occasionally took the seminar as a whole, that was a big affair.

Weiner:

He treated them generously?

Bloch:

He treated them very generously. He was a well-to-do man. But then it required a large table.

Weiner:

This was in San Francisco?

Bloch:

Yes.

Weiner:

I think Serber in his remarks at the memorial ceremony for Oppenheimer recalled that.

Bloch:

Yes, we went to San Francisco. We went to a fish place down in the harbor. Oppenheimer had all kinds of exotic restaurants that he knew and liked to go to.

Weiner:

Getting back now to Stanford, when did the work on the cyclotron here start and how did you become involved?

Bloch:

Now that's much, much later. You see, in my previous discussion I still had more or less the time '34-'35 in mind. By the way, I was also in Europe in

between several times. If you want to go to the cyclotron, you're making a jump of at least five years.

Weiner:

I don't want to do that? I want to go chronologically. I asked you about how you began to get in touch with other people. We talked about contacts with Berkeley, and we talked a little bit about Pasadena, and then we talked about going east for the meeting. What about other people from Europe coming here, having come to the U. S: either to live or to visit.

Bloch:

Well, I certainly remember later that we did receive visitors. When we got our first visitor from Europe here I don't quite recall. I recall, but that was much later, a visit from Niels Bohr here in 1939, but that stands out in my memory because it was Niels Bohr. I'm sure we had other visitors here before. I think I would like to tell you a little bit now about the years between '34 and approximately '39, because it was a rather important development for myself. In the first place, of course, I immediately became interested, because I was in this environment, in quite different

problems than I had worked on before. My first paper, in fact, here was written on X-rays. It had something to do with the work, which was done here, because people discussed things and I felt I had the theoretical answer to that. Then, at the same time, and very shortly afterwards, I started to do experimental work myself. Now, this, I'm sure, is a development which would not, or hardly that early, have happened to me in Europe. I told you, and, in fact, thought it was a good thing that the distinction between experiment and theory was not as sharp here as it was in Europe.

Of course, in Europe, Fermi was the only outstanding, shining example of a man who did both experiment and theory, and I remember one of these casual remarks that Fermi once in a while made. He said to me in Rome once: "You know, you should sometimes do some experiments: It's really a lot of fun." That little remark of Fermi's stuck somehow in the back of my mind. When I came to this country then I realized that I wanted to do experiments: Let me put it a little bit clearer. I was here from '34 until the summer of '35 practically without interruption. Then in '35 I went to the summer school in Ann Arbor, where Fermi was again, and from there went

to Europe really to visit my parents and, in fact, didn't come back until the winter of '35. I had the summer and then the first quarter of the fall, so I had a half a year free.

Weiner:

Why did you have it free?

Bloch:

Well, the summer was free anyway, and the people just were generous here and said: "You don't have to teach the first quarter." Things were quite informal here. So I spent half a year away. Then when I was in Europe, I thought of course what one should do in physics, what was the interesting field; and neutron physics was the sort of thing that interested me at that time. Fermi had just started his important work. So when I came back I approached one of my colleagues, Bradbury, who is now director of Los Alamos, and I suggested to him that we should build a neutron source. We had no other equipment except X-ray equipment, which provided 200,000 volts: Now, 200,000 volts was a marginal but possible case in which one could produce neutrons by accelerating deuterons rather than electrons. So at least we had

the voltage for that. And I suggested at that point when I came back that we should try to produce some neutrons, do something with neutrons. I thought that was a new and interesting field. But before we could do that, we had to build an ion source to produce ionized deuterium. Bradbury was an expert in gas discharges; he had studied with Loeb. So I said to him: "Let's get together and build an ion source." I had never, except in my early student days when I did some elementary physics lab, had any experimental experience. But it was a great experience then to work with Bradbury and do some of the really low-brow work, soldering and whatever is necessary; and so I did acquire at that point—and I must say with great joy—the elements of experimental physics. And we did build a neutron source. We installed our source, our atom source, which eventually was built in one of the X-ray tubes, and by very primitive means convinced ourselves that we indeed saw some neutrons. Now, of course, that wasn't a great achievement, but nevertheless it was very exhilarating.

Now, that had a good deal of influence on my work. During that time, there were several things which I did. One of them turned out rather important later

on, and that was during a visit of Nordsieck, Arnold Nordsieck. I don't know whether the name means anything to you.

Weiner:

Yes. You published some things with him.

Bloch:

He went to Leipzig after I left, and then from Leipzig he came here. And we worked together at that time on a rather strange difficulty of electrodynamics and solved it. There is a still famous paper of Nordsieck's and mine, which we wrote at that time. At the same time I was doing the neutron work. I began to think what one should do with neutrons when one had them. Then I had also rather an important idea at that point, and that was that one could produce polarized neutrons: I don't know whether you know what that means.

Weiner:

Yes, this is '36, your paper in '36.

Bloch:

I wrote a paper at that time; you're quite right. In fact, the Nordsieck paper and the neutron paper were my most important papers at that time. They came practically at the same time.

Weiner:

I think I have reference to that. Had he come as a staff member here?

Bloch:

No. He came with a fellowship—I don't remember which fellowship.

Weiner:

That paper came later with Nordsieck, in 1937.

Bloch:

'37, right.

Weiner:

And your paper on polarization was in 1936?

Bloch:

Yes. That came right when I came back. When I came back from Europe in early '36 I wrote this paper, and also I started to think about the other problem. I wrote this paper on neutron polarization, and we started to work together—Nordsieck and I—on that other problem. He came here and said: "This is a scandal. There is this problem, and one should be able to solve it." So he convinced me, and we worked together on that. Now, about visits, I would like to say the following: It so happened that there was money for a visitor at Stanford during the summer. The summer was free, and nevertheless there was some money available. So I suggested to the people that they should use that money to invite some rather outstanding people to spend the summer here, knowing that it is a very nice place. The first man whom I brought here was Gamow. Gamow was here the summer of '36 and was of course a very stimulating, interesting man who brought a breath of fresh air into it. We were very close; we saw each other all the time.

Then the next visitor was Fermi, who was here the summer of '37. He came from Ann Arbor upon my

invitation. He spent, I think, about four weeks here: And of course that was marvelous. I think I got more out of Fermi than I ever got in Rome, because he was here for no other purpose than talking, and we went together to the Sierras and to the coast and so on.

Weiner:

Was his wife with him on that trip?

Bloch:

No. It was at that time that I sensed that Fermi was moving away from Fascism and Italy. Although I have no proof of that, I'm pretty sure that at that time it was pretty well decided that he was not going to stay.

Weiner:

I recall somewhere the story about the Burma Shave signs. I wonder where I heard that.

Bloch:

Probably in Mrs. Fermi's book someplace. Yes, *Atoms in the Family*. Yes, she was quite shocked about that.

Weiner:

I don't recall how that went. I remember vaguely.

Bloch:

Fermi was here in the summer of '37. He went back to New York; I went also to New York but via the Canal. Then we met in New York, Fermi and I, and we traveled together on an Italian boat to Naples. Then Mrs. Fermi met him in Naples, and I proceeded on my own to Rome. But then we drove from Rome to Florence together. I drove with the Fermis. And there were these signs which said "Mussolini is always right" and "Believe, Obey, Fight" and all these slogans, you see. And Fermi read them and always said, afterwards: "Burma Shave" because he had seen these signs. It was clear that his respect for the regime was gone by that time. But to Mrs. Fermi it was a surprise. I'm sure this happened to him during the time he spent in America. Well, there was the Abyssinian war, which Fermi already didn't like, and he really moved away from it then.

Weiner:

Then racial laws came out in Italy, too, about that time.

Bloch:

Yes, they began to come out. Strangely, although Fermi was not of Jewish descent whereas Mrs. Fermi was, he was more sensitive to that than she was.

Weiner:

There are a couple of things that you touched on. I want to get back to your main line of research. But before we do, you opened up some interesting things that I'd like to get to because they're chronologically appropriate. You went in '35 and spent a good many months in Europe?

Bloch:

First I was in Ann Arbor, Michigan.

Weiner:

That was for the summer school.

Bloch:

The summer school. I think I went to Europe for about four months.

Weiner:

I first wanted to ask about the summer school, if you have any special impressions of that. I know the atmosphere generally was very informal.

Bloch:

Oh, that was a great institution. That was a gathering place for the best people—partly from Europe, partly from here. It must have been due to the presence of Uhlenbeck in Ann Arbor that they had arranged that mostly. And they had people coming there. Fermi, of course, was always a mainstay of interest, and Segre was there again, and we were there together at some time. I think Willis Lamb was there. I'm not quite sure. But there were many people there. They were most interesting.

Weiner:

Was there anything like that in Europe?

Bloch:

No, not that I recall except of course for the meetings in Copenhagen, yes. The Copenhagen meetings were much the same, the ones that Niels Bohr convened. Then there were usually certain special topics that were discussed at that time. In fact, the Copenhagen meetings probably historically were much more important than the Michigan meetings, but they were similar.

Weiner:

But the Michigan meeting lasted longer generally because it was a summer session, six weeks or something.

Bloch:

Yes, four weeks, six weeks, something like that.

Weiner:

And from there you proceeded to Europe. Where did you go?

Bloch:

Well, are you speaking now about that first visit?

Weiner:

That's right.

Bloch:

In '35. Oh, as far as I remember, I went directly to Switzerland.

Weiner:

To see your family.

Bloch:

Yes. I spent that time in Switzerland.

Weiner:

You didn't go to Germany at all?

Bloch:

No, no. I may have visited Austria. I probably did because my grandmother lived there.

Weiner:

You mentioned, though, that during this period you became more and more interested in neutron work, which seemed to be occupying many people at that

time—for good reason. How did that come about? Did it come about through conversations with individuals in Europe?

Bloch:

Yes, I believe so. I have the distinct notion that when I came back from Europe I had it formulated in my mind that that was the thing to do. But I cannot pinpoint it now, just who it was with whom I talked. Wait a minute. In 1935 I was in Copenhagen. Yes, now I remember. For the 50th anniversary of Niels Bohr we went to Copenhagen. There was a meeting. That was during that summer. At that time, probably, it was mainly through Bohr. I'm sure that this idea of neutron work originated on that occasion.

Weiner:

Do you recall anything about that 50th anniversary celebration?

Bloch:

Well, not in detail. I mean it was one of these wonderfully friendly things. We met at Niels Bohr's Institute. There were many people there. I saw my

old friends in Copenhagen. Do you mean from the point of view of physics?

Weiner:

Yes.

Bloch:

I'm not quite sure whether I heard there for the first time about, for example, the experiment of Cockcroft and Walton. I'm sorry—I should have studied the date.

Weiner:

That '32. Artificial radioactivity came in 1934.

Bloch:

I'm sorry; I can't recall in detail what felt—

Weiner:

Your main contacts then were family and then Copenhagen.

Bloch:

Well, then, I'm sure I saw Pauli. Yes, I know I saw Pauli in Zurich, too. He was in Zurich. So I did meet some of the physicists.

Weiner:

Did you get any impression of change in the general social-political situation compared to the year earlier, more of a sense of despair and desperation?

Bloch:

Surely the awareness of the seriousness of what was happening in Germany had more deeply penetrated. You see, the problem is that when I came back to Europe I was, so to say, a different person myself. That is, I came from America. I had already spent a year and a half in America, and so probably when I saw things in a different light it may partly have been my own reaction to them, and it may partly have been objective. I can't say that with too great certainty.

Weiner:

But it was sort of a catching-your-breath period after...

Bloch:

I don't think I had the feeling in '35 that this was going to probably be my last visit to Europe. It wasn't either. But by my second visit I was pretty sure that was just about the end. Of course, on the other hand, I do remember I think I came back with the feeling this was the last. This was not going to last much longer—the sort of normal way as you met it outside of Germany. Pauli was great in trying to imagine that these were all not such important events that life would go on as usual.

Weiner:

When you came back and started on the neutron work did you have your old theme of magnetism in mind? What were you trying to produce neutrons for?

Bloch:

Well, you see, the idea of neutron polarization was done by magnetic means. But the reason you can polarize neutrons is because it was assumed and pretty well experimentally supported that neutrons had a magnetic moment, and, in fact, the polarization was done by passing them through magnetized iron. So my early work on ferromagnetism was directly linked with this. In fact, I might not have had this idea of polarizing neutrons that way if I had not known so much about ferromagnetism from my earlier work. So there was a strange interplay between my solid-state work and my neutron work.

Weiner:

That's interesting. What I meant, though, was when you decided to produce neutrons, it wasn't just for the sake of producing them; it was for this...

Bloch:

No. At that point I just simply wanted to produce neutrons.

Weiner:

You wanted to make neutrons.

Bloch:

First make neutrons and then once one has a neutron source, then one can see. Well, I took pretty much the attitude that people took here towards X-rays. I mean you have to have X-rays. You wanted to do X-ray work, so you had to have X-rays. Then you were able to do various things with X-rays. No, I was not too clear about it at that point. I mean it was clear it was a big field. There was much to do. I was pretty sure that once we had neutrons we would do something with them. Now, it so happened that this was a rather original idea—on neutron polarization. I was very pleased. I felt that the justification for starting with neutrons would come later.

Incidentally, that experiment to polarize neutrons was never done here. Our neutron source was never worth much. To me it served only as a stimulus to ideas. It was much too weak.

Weiner:

It was done by the people at Columbia and other places.

Bloch:

Right.

Weiner:

I'll talk to you later about the so-called Dunning effect.

Bloch:

I don't know whether you want me to fill up this gap now.

Weiner:

I do want you to fill it up, but I also wanted to know how the polarization ideas came about and how you developed it. It's one thing to say what the ideas are about and what form they finally took. But did it come in stages?

Bloch:

Well, you know, the exact circumstances on how an idea occurs is never too clear. I may say that the idea that the neutron may have a magnetic moment was in my mind and was quite old. In fact, I had already mentioned it to Heisenberg in Leipzig, and Heisenberg thought it was something worth pursuing. In fact, I had a very erroneous argument at that time, which made me think that the sign of the neutron moment would be opposite to that of the proton. But then, of course, experiments were done by Stern when he was still in Hamburg which showed that both the proton and the deuteron had a magnetic moment, and from that, one could conclude with some certainty, since the deuteron consisted clearly of a proton and neutron, that the neutron would have a magnetic moment. So that pleased me because I had sort of dreamt before that it might have one. Now, I am sure that we did not start neutrons here because of this idea of the magnetic moment of the neutron, which I had before. But once we started to do that, then I started to think that maybe we could do something more about this interesting aspect of neutrons here—the

magnetic moment. And then at that point I think somehow this idea occurred.

Weiner:

You suggested how it could be.

Bloch:

Oh, yes, I wrote a paper on it.

Weiner:

The 1936 paper.

Bloch:

Right.

Weiner:

Although the work itself wasn't done. In the course of writing the paper were you at the same time trying to work out your prediction?

Bloch:

Yes, I was hoping we could do it here. But I don't know whether we had already our ion source functioning at that time. We were way behind.

Probably if I had known that we could do the experiment the next week, I might have just done the experiment and then published the paper.

Weiner:

Published the theory and the experiment.

Bloch:

Yes. But realizing that the experiments were far in the remote future here, I published it anyway. And I wasn't sorry that somebody else did it—not at all.

Weiner:

What was the reaction to the paper? Were you in contact with anyone?

Bloch:

Oh, yes. Well, it was noticed at that time. In fact, Schwinger quoted it in his very first paper. As a very young man he wrote a commentary to my paper pointing out that there was something which was not quite correct in my paper, and he was right.

Weiner:

Was this the one he did with Rarita?

Bloch:

No. That was Schwinger alone. It was his first paper. He was 18 years old at that time. He noticed that the way I had treated interaction between neutrons and magnetism, the magnetic shell, was not quite correct, so he did it in a different manner and came to a slightly different result. He was very proud of it and rightly so because he was very young.

Weiner:

Did he get in touch with you personally?

Bloch:

Yes, of course and with Rabi. I spoke to Rabi. I knew Rabi also by that time and saw a great deal of him in New York. That paper was well known. It may have been Rabi or Schwinger or somebody who went to Dunning and said: "Why don't you try it? You have a neutron source." I don't know. As I say, physicists knew each other? The group was very small, and everybody knew what everybody else was thinking.

Weiner:

Once this paper was published, once you had completed the paper, was it clear then that your next piece of work would be to follow this up and to try first of all to do it and then to...?

Bloch:

Yes. I don't quite recall the chronology. Yes, I'm sure. I told that to Bradbury. I said, "By the way, once we get our neutrons going I have a nice experiment we could do with them." He was of course delighted. And that was clearly our intent. But then Dunning did it before. But then during the visit of Fermi in 1937, I had another idea. In fact, it was practically simultaneous with Rabi's idea. I'm afraid it's getting too technical.

Weiner:

Don't worry.

Bloch:

You see; the polarization of neutrons demonstrated that indeed free neutrons had a magnetic moment and approximately gave how big it is. But then I felt

that one should make an exact measurement of the neutron moment. And that idea occurred to me either shortly before or during the time when Fermi was here in '37. I sort of remember that I mentioned it to him orally. That was a much more complicated arrangement that had to do with polarizing and analyzing a neutron beam with a constant magnetic field and a radio frequency field in between. It is closely related to the technique that Rabi then used for molecular beams, and in fact Rabi wrote his first paper in 1937 pointing out that a combination of a constant magnetic field and an oscillating magnetic field could be used to measure magnetic moments of nuclei. That's what he was interested in, you see. And I had this same idea independently, but I didn't publish it.

Weiner:

For neutrons?

Bloch:

For neutrons. So at that time Fermi knew certainly that I was out then not anymore for the scattering experiments, which I believe by that time had been done anyway—the polarization experiments had

been done—but for exact measurement. And that, as you remember, I did later in 1939 with Alvarez: It was clear that our source was not sufficient. We tried first with our artificial source, then tried with a radium source, which was also still too weak. That brings me eventually to the cyclotron. But let me just finish with the summer visitors because you asked me. They were quite a regular feature. So in 1937, as I told you, I went to Europe with Fermi. Then I stayed there until the spring of '38.

Weiner:

Again one of these long trips. What did you do then in that whole period in Europe?

Bloch:

What did I do? I'm not sure that I was very productive at that time.

Weiner:

In '37 of course there was the paper with Nordsieck that was done here.

Bloch:

That was already finished. One thing I do remember I did in '37—I went to the Galvani Conference in Bologna. There was a big meeting. The King of Italy was there and so on. I met lots of people there. I saw Heisenberg also there for the last time. It was long before the war, and I tried to persuade him to leave Germany. It seemed at that time quite possible that he might. He unfortunately decided not to do it anyway.

Weiner:

Did he give the basis of his reluctance?

Bloch:

Yes, he had difficulties with the Nazis at that time, partly because of his former Jewish collaborators, and he was very angry at them. He needed a little push. But I think Debye was really the one who persuaded him that he should not leave, although Debye himself then left. Anyway these are matters, which I don't want to go into too much. Well, that's one thing I remember I did at this time—visit this conference in Bologna. Otherwise, I don't remember

what I did in detail. I think I was mostly at home with my parents and went to the colloquia at the university in Zurich.

Weiner:

Pauli was right there.

Bloch:

Pauli was there all the time. I saw him a great deal.

Weiner:

It's misleading then when one looks at your biography and your resume to assume the European contacts ceased, because here you were very close to Pauli in '35 and '37.

Bloch:

Yes.

Weiner:

Do you recall, though, what work you were doing then?

Bloch:

Frankly I don't. I think I must have been in some kind of transition period. I must have been still thinking of this neutron work, because I remember that shortly afterwards when I came back we started to do the experiments here—the measurement of the neutron. That was in '38. During that summer Rabi was here. I mentioned to you Gamow first, then Fermi, and in '38 Rabi was here. Then, of course, we talked a great deal about this kind of thing. In fact, we had that experiment going and Rabi came every morning and saw how this was going.

Weiner:

Was this the Alvarez experiment?

Bloch:

No, no, we did it here. We had a radium source and did it with very primitive means and hoped that we could do it and found out that it wasn't possible.

Weiner:

These summer visitors—was there any formal summer school established?

Bloch:

No, they gave some lectures. I don't know what Fermi talked about. Rabi talked about molecular beams. But then people came from Berkeley. In fact, I should say that these sessions here became rather well known. Willis Lamb spent several summers here. He came here for this occasion when Fermi was here and when Rabi was here and so forth:

Weiner:

Didn't Weisskopf come?

Bloch:

Yes, I wanted to say that. Weisskopf came in '39. Weisskopf was also here as a visitor before, but we invited Weisskopf in '39, I believe, and then Bethe: That was in '40. But we had a little summer school, so to say, going on, although it was always centered around one single visitor.

Weiner:

That clarifies it, because people have mentioned to me: "Well, I went out there for the Stanford summer school." And I didn't know what this meant. They

were using the same kind of term that was used at Michigan, but they didn't mean the same thing.

Bloch:

It wasn't a real summer school. Gradually it grew as more people came, but it was always centered around the one man whom we invited. It was a godsend. The people here didn't care much about that, but when I found there was money available for inviting a man for the summer, I said, "Let's do it."

Weiner:

What about other people from Europe who were in need of positions? I know that Marcel Schein came out once.

Bloch:

Yes. In fact, I drove with him through the country once, and he was looking for some sort of job at that time. Then another man I brought here (he was also a student of Heisenberg's) was Arnold Siegert. He is now at Northwestern. He's quite well known, especially for his work in statistics. He came here as an assistant. That was all we could offer him, and he was very happy to come here as an assistant. He was

a bachelor; had the minimum salary. That was the same time that Nordsieck was here. That must have been '36 that he came. Then, of course, gradually I did meet other people who came over here. As I said, Weisskopf. There was Gamow. I'd count Gamow among these people. He came from Russia—fled Russia. Gamow spent the summer here in '36.

Weiner:

He came in '34 for the Michigan summer school and then came out to Berkeley because he thought Lawrence could get him something. I'm just wondering if it was on that trip. That would have been '34.

Bloch:

No, that was before. He was here later?

Weiner:

He had already become established at Washington.

Bloch:

Yes, he was already at Washington.

Weiner:

I talked with him in April. One other point on this before we get back to the main line of research and the new thing, the cyclotron. I came across this letter that you had written where apparently you were concerned about the deteriorating situation in Europe for Jewish scientists especially. This is a copy to Tolman. He then sent this copy to Millikan, asking for Millikan's help. The date is '38.

Bloch:

It's strange—I had entirely forgotten that letter. I wonder; did Millikan do something about this?

Weiner:

Yes. This is another project I've been tracing.

Bloch:

Do you have the answer from him?

Weiner:

The answer to you? No, I have Tolman's covering letter, where he says: "I'm sending you herewith a copy of Schrodinger's letter," which I have here. He

said, "I'm also enclosing a copy of a letter from Bloch of Stanford which is self-explanatory. He asks me in the letter to approach you with regard to sponsoring some action as to the persecution of Jews in Germany. But I see no reason why I should not do this best by showing you Bloch's actual letter." Now, I don't know what role this played in it, but later that year Millikan was involved with a number of American university presidents.

Bloch:

Was that handwritten?

Weiner:

Apparently it was, because this is a typed copy: The only reason he would copy it would be if the original was handwritten. Then this would be easier to read, assuming that Millikan is very busy and so forth. Then there's also a copy of the letter from Schrodinger, which later on I'll check...

Bloch:

To Millikan?

Weiner:

To Tolman, a very interesting letter about what was going on in Europe.

Bloch:

That was written from England?

Weiner:

Yes, from Oxford. Anyway, so apparently you were involved in that. You don't recall the letter, but does it stimulate any other recollections of what prompted you to do this?

Bloch:

No, I'm ashamed to say I don't remember details. I remember my concern about what was happening in Europe. I dreamt about it, thought about it.

Weiner:

Of course you had been back in '37, so you had even more chance to see.

Bloch:

Oh, yes. The impending doom was clear. And I think when I left in '38 then I had still visited once more my grandmother in Vienna. It was just shortly before the Nazis took over. I remember I left Vienna with great depression, trying to tell my relatives there to take anything they can and leave, and they did not want to. They left eventually. This letter is earlier. I see, November '38. Of course, that's right. I came back from Europe in the spring of '38. At that time Austria was already overrun, too.

Weiner:

Things had really reached big proportions. It wasn't a question of a few people, but of thousands. Getting back to the work underway here, you mentioned that when Rabi came out in '38 he was watching with interest the development of the experiment. How did that proceed from that point?

Bloch:

Well, then I realized, and it may well have been Rabi's advice, that our source would never do, and that the right thing would be to do it on the Berkeley

cyclotron. So I think it was in the fall of '38 that I spoke to Lawrence, and he was very kind and understanding and also felt this would be an important experiment. I don't know whether he asked Alvarez but Alvarez said he would like to do it with me. So that started my collaboration with Alvarez. I think it started in the fall of '38.

Weiner:

And this was with the 37-inch cyclotron.

Bloch:

That was with the 37-inch cyclotron in Berkeley. And then I commuted, practically, between Stanford and Berkeley. Once in a while I spent a night up at Segre's in Berkeley, but usually I came back and gave my lectures. But then I did not pursue the experimental work here anymore. I worked with Alvarez.

Weiner:

In a sense it was very fortunate, because he was probably the only one around who had a neutron beam.

Bloch:

Oh, surely, of course. That was practically the only working cyclotron in the world. It didn't work too well either. Once in a while it worked.

Weiner:

What about the work on it? Of course, there must have been interruptions because of the fact it didn't work, and it was used for other purposes.

Bloch:

Oh, yes. There were frequent interruptions. In fact, once we had our apparatus built, I remember for weeks and weeks what we did mostly was sit around waiting until the cyclotron beam was on, which sometimes happened once a day. Sometimes it didn't happen for three days. Sometimes it lasted five minutes, and sometimes it lasted three hours. Whenever it was ready we had to work?

Weiner:

I talked with Alvarez, you know, and got some of the background. He explained that you'd have to

wait when the machine was being used for cancer treatments.

Bloch:

Yes. Besides the machine wasn't really working very well at that time.

Weiner:

We also have the notebooks, by the way. He gave us the notebooks of the experiments, which we have preserved in our archives. There was a photograph on the cover of you sleeping on... I don't know what. We looked carefully at the photograph, and it looks as if it's on a boat.

Bloch:

It might have been the ferryboat between Oakland and San Francisco.

Weiner:

It could be. There's supposed to be something symbolic about the picture. I never asked him why that particular picture. And then the data in the book is in your hand and in his as well.

Bloch:

Yes. So that went on through all of the spring of '39 and then also in the summer of '39. That's when we finally finished the experiment and got our results.

Weiner:

Did somebody named Laslett work with you, Jackson Laslett?

Bloch:

Yes. Oh, there were a lot of people there. I don't think Jackson was particularly close to our experiment. He was one of the people. I mean there was a whole crew running the cyclotron.

Weiner:

Was this your first experience with such large-scale research facilities?

Bloch:

Yes.

Weiner:

Well, your experience was limited anyway.

Bloch:

Yes, of course. Yes, it was. It wouldn't be considered large-scale nowadays, but at that time it was, yes. Lawrence was most generous. I think he gave us as much time as could be made available at that time. He really felt that this was one of the really good experiments to be done with the cyclotron. There were not too many at that time.

Weiner:

Did you think that, given a working machine and given access to it, it would have taken just a short time to do? Did you think that this was a relatively simple matter?

Bloch:

Well, I don't know. I probably underestimated the difficulties when we started, but then gradually it was clear that it was not so simple. This is always true in research. I think very few people can make a proper forecast of how long it will take; because you never know what difficulties you will run into. But then we had, of course, always our little encouragements and discouragements and so on.

Weiner:

In the course of this, Alvarez mentions the attempt to find the effect that Dunning and others had reported. It had been reported in three places, based on your 1936 paper, and yet you had difficulty in finding it.

Bloch:

First of all, we had difficulties in finding it at all, really. We didn't quite realize, but we suspected that the iron had to be very thoroughly magnetized before the effect would show up at all. The first thing was, as you say, and as Luis remembered correctly, to simply reestablish the Dunning effect, so to say. That is, to find that there is indeed an effect upon the transmission of neutrons by the magnetization of iron. Then once we had that, I think that from then on it was not so hard anymore. But we had to do that first; that's right.

Weiner:

You were convinced there was, because you were the one who had written about it in the first place. But apparently you weren't convinced that Dunning had seen it.

Bloch:

I was not quite sure, and I'm not quite sure to this day whether he had seen it. I'm sure he thought he had seen it. It was marginal, and in our case it was marginal, but it was clear enough that finally we were convinced that there was an effect.

Weiner:

And then you could proceed.

Bloch:

And then we could proceed.

Weiner:

So that added to the time involved in the experiment.

Bloch:

And then, of course, after that experiment was finished in Berkeley, then I realized that there was more to be done along those same lines; and I could not keep on commuting to Berkeley back and forth. Therefore, we then decided to build our own cyclotron.

Weiner:

You mean the pressure for the cyclotron here, at least from your point of view, was because you had a specific research need in mind.

Bloch:

Of course. Definitely.

Weiner:

It's quite different than how it developed in other places.

Bloch:

Quite definitely. Yes, yes. I spoke to Lawrence about it, and he said, "Yes, I think you should have your own cyclotron. We'll give you all the help you need."

Weiner:

Was there anybody else here who felt that way about it?

Bloch:

Yes, particularly Staub. He is a Swiss. He came here in '38. He was at Caltech and then came here, and he and I were very close collaborators. We built that cyclotron together. That was one of the first things we did when he came here. He's also mentioned in Mrs. Fermi's book.

Weiner:

Did he come as a refugee?

Bloch:

No. He was a Swiss and not a Jew. He came here on some sort of an exchange fellowship or so, spent a year in Caltech at Pasadena, and probably even thought at that time he would go back. But then I told him: "Why don't you come here?" and so he stayed here until 1949.

Weiner:

Yes, there's a great picture—I don't know whether you're on it or not; I think you may be—at Los Alamos with Bethe and Staub and perhaps you and Weisskopf on top of a mountain skiing.*

<I>(*Picture deposited in the Archives of the Center for History and Philosophy of Physics by Emilio Segre shows the following group skiing at Los Alamos in 1943: Segre, Fermi, Bethe, Staub, Weisskopf, Mrs. Staub and Mrs. Segre thermodynamics.)</I>

Bloch:

It could be. I didn't stay in Los Alamos very long.

Weiner:

This was very early:

Bloch:

It could have been. Yes, Staub and I went up there together.

Weiner:

So you built it together. First of all, how does one go about convincing the university that they need a cyclotron?

Bloch:

Oh, well, the only thing is to get the money. Now, in that respect I had the help of Rabi—I think it was in

the fall of '39—who took me to see Dr. Weaver, who later was president of the Rockefeller Foundation. Rabi knew him. I'm sure it was due to Rabi's support that they gave me \$4000. At that time the Rockefeller Foundation almost entirely sponsored medical work, but nevertheless, they gave me that. So then I went back to Stanford and saw the president and said: "I want to build a cyclotron." He said: "Who will raise the money?" I said, "I've just raised \$4000, but I don't think it will do." We made a very close estimate and we thought it would run over \$5000. He said, "I'm sorry—I don't have any money." This was Ray Lyman Wilbur. So I said to him: "Do you think I should start anyhow?" And sort of with a twinkle in his eye he said: "Well, if I were you, I would start anyway." Then somehow we financed it. We didn't get a real gift from the University, but we got a little bit of money here and a little bit there. We scraped together the last thousand dollars. There was no need to convince the University. You could do anything you wanted as long as it didn't cost any money.

Weiner:

What argument did you use with the Rockefeller Foundation? Did you use the medical argument at all?

Bloch:

No, no.

Weiner:

You could Berkeley used it.

Bloch:

No. Rabi knew exactly what it was about, and Rabi was very much interested in this experiment. No, we told them exactly the truth. That's what it was for. And I had done the experiment already before, so it was clear that there was something in it.

Weiner:

But you don't build a cyclotron just for one experiment or one series of experiments.

Bloch:

Well, it was not one experiment. There were ideas. In fact, they came up later and were also used later for using the magnetic scattering altogether for determining structure in iron. This is work which is done at Oak Ridge even now. Of course, they have much better neutron sources. It was not this one experiment. I considered that more or less finished except that I thought it could be done better. It was clear there was a whole new field.

Weiner:

But basically you would say that this cyclotron was designed as a producer of neutron sources. This is the thing you had in mind.

Bloch:

As a producer of neutron sources for the purposes of magnetic study of them. I was quite specific. The Rockefeller people knew that.

Weiner:

Did that have anything to do with any special design considerations?

Bloch:

Not particularly, no. We just wanted, for the money which we had, to get the strongest neutron source we could, and it was practically a copy of one of the earlier cyclotrons.

Weiner:

How did Lawrence help you on it?

Bloch:

Well, he helped, of course, with advice. He also let us have one of their old tanks. It didn't help us much because it leaked so very much we had to build our own anyhow: He did whatever he could, but I would say the most important thing was the moral encouragement. In material things, although he meant well, the things we got from him were not too excellent.

Weiner:

You had your own magnets.

Bloch:

We had to build them, build them with our own hands. We bought steel, the cheapest kind of steel.

Weiner:

I didn't know that you were involved in that day-to-day work.

Bloch:

Oh, very much so.

Weiner:

And who else?

Bloch:

Staub and Stevens, who is now at Pennsylvania.

Weiner:

Was he a student at the time?

Bloch:

No, he was an instructor. I think we had occasional help from students also.

Weiner:

What was its size?

Bloch:

Well, it was about 20 inch or so. We accelerated deuterons to about 2 million volts. It was the simplest design we could find. It was air-cooled. That was an innovation because our water was full of chlorine and it would corrode the cooling pipes, so we had an air-cooling system:

Weiner:

I'd like to find out when you started construction. You told me about getting the money, but it wasn't clear as to the date.

Bloch:

In '39, late '39.

Weiner:

I see. And then you started construction right away.

Bloch:

Pretty soon, yes. I suppose we started then designing, started to buy material, and so forth. It took a long time: It was finished, I believe, in early '41.

Weiner:

So you didn't have much time to use it once it was finished.

Bloch:

No, not much. [Pause in recording]

Weiner:

We're resuming now after a break for lunch and a rest. When we left off, we were talking about the work with the cyclotron, which you said just got started before the war. We agreed that it would be good to stop at that point, and then to pick it up and to see if there was anything else we missed in the late '30s and early '40s, and if not, then to take the next step.

Bloch:

Well, I just wanted to tell you there was some work we could do with the cyclotron before we started war work. Is that what you wanted?

Weiner:

Yes, I wanted to find out what happened once it was working.

Bloch:

Well, the first thing we did was to very greatly improve the polarization effect: You recall I told you this morning that Alvarez and I had great difficulties in the beginning reestablishing even what Dunning had done, the very small effect. But we worked on that anyhow. Well, when our cyclotron was functioning in '41, the first thing we did was to go back and investigate what it was that caused these rather poor polarization effects. And we found, although there was some theoretical guidance on that that one had to push the saturation of the iron very, very close to completeness. At that point then we built special magnets for that, and then we achieved effects of the order of 20% instead of one or two

percent: That was a systematic investigation we did, so that we knew from then on one could get much higher polarization effects? But then the Manhattan Project got underway, and I was in this early group with Bethe and Teller and Oppenheimer.

Oppenheimer called me up one day and told me I should come and see him in Berkeley. I realized then that things had completely changed, and we were told then the first problems about the atom bomb.

Weiner:

Before that, how did you receive the news about the discovery of fission?

Bloch:

Oh, that was very interesting. Again, I got a telephone call from Oppenheimer. Wait a minute. The news of fission I think I read in *Naturwissenschaften*. You mean Hahn's paper?

Weiner:

Hahn and then subsequently Frisch and Meitner.

Bloch:

That I'm not quite sure whether I saw, but I saw my first evidence in Berkeley. Then I got a telephone call from Oppenheimer that I should come up and see something. Gentner was there at that time from Germany, and they had gotten in emulsions these beautiful recoils of fission fragments simply by neutron irradiation.

Weiner:

Did they call them splinters then?

Bloch:

I don't remember what they were called. Anyhow that was the first time I actually saw the fission process with my eyes.

Weiner:

This would have been in '39.

Bloch:

That was in '39 or perhaps in early '40.

Weiner:

Had you attended any of the Washington theoretical meetings, the ones that Gamow and Teller were involved in?

Bloch:

Yes, I was in Washington, too, at some of the meetings. That was earlier, though. That was not on the atom bomb.

Weiner:

No, they were the ones at George Washington University and Carnegie Institution.

Bloch:

I'm pretty sure that I was at least at one of them, yes.

Weiner:

But you apparently were not at the one, I guess, in the early part of '39 where this was discussed?

Bloch:

No, I wasn't. But to come back to that now, Bethe was here in the summer of '40. Then he came back

in the summer of '41. And it was Bethe and Teller and someone else and myself. We learned then about this so-called Manhattan work going on, and we were all asked to join it and do whatever we could. And it was assigned to us here at Stanford to determine the energy distribution of the neutrons emitted in the fission process, which was a rather important question for the chain reaction. This is what we did. The laboratory — down in a light well under a glass roof—was closed. It was done under strict security regulations.

Weiner:

By this time you were a citizen?

Bloch:

I became a citizen in '39, yes.

Weiner:

You got married then.

Bloch:

I got married in '40.

Weiner:

And your wife was a physicist.

Bloch:

Yes, that's right. She was doing research at MIT before we married.

Weiner:

Had she come from Germany, too?

Bloch:

Yes. In fact, she's from Gottingen. She got her doctor's with Victor Moritz Goldschmidt. He was a geophysicist.

Weiner:

When was this?

Bloch:

She got her degree in '38, shortly before Goldschmidt left Germany. He went then to Norway.

Weiner:

And then she left at about this time?

Bloch:

Then she left at that time. Then she was also in Copenhagen, and then she was in Geneva, but we didn't meet at that time. We met over here.

Weiner:

You were on a trip to MIT when you met her?

Bloch:

I met her actually at the house of friends of ours in New York.

Weiner:

Well, getting back to the beginning of the work on campus, the war work.

Bloch:

We investigated that. Already at that time we had shipped some rather strategic materials—very, very pure graphite blocks which we needed. Some of them were used for the plant in Chicago. Then we

got the first enriched sample of Uranium 235 from Berkeley, very rare, too, but we needed that. Well, we finished that work, and we got some quite interesting results out of it. It turned out that the fission spectrum extended rather to higher energies than one thought. It was pretty well the Maxwell distribution, but extended to something like 5 or 6 million volts, whereas people felt perhaps 2 million volts would be the results. So I think it was not without significance. But of course that was classified and was not published. Already before that was finished, the Los Alamos project got underway. We were asked to go there. I must say I had some hesitations before going there. I wasn't sure, first of all, what I would do there and whether I could really live in this military atmosphere. But nevertheless I felt it was my duty at least to try. And Staub went with me, too.

Weiner:

Who was the one that recruited you? Was it Oppenheimer?

Bloch:

It was Oppenheimer. So then I did go to Los Alamos and started early. I found then that the nuclear physics that was going on there, special theoretical work, really didn't interest me. I didn't fit in there. That was Bethe's group, and he had his people compute it. I found that it was not what I wanted to do. I got interested in the implosion, in Neddermeyer's group: Neddermeyer initiated this idea.

Weiner:

This is the Neddermeyer who was at Caltech earlier?

Bloch:

Seth Neddermeyer, right. And he had worked, I think, at the Naval Ordnance Laboratory and knew about what explosives could do, and had done already preliminary experiments on implosion. I did some work on that, both theoretical and experimental, to show—people were not convinced of it; I must say I had very little doubt about it—that the velocities and pressures which one would get were indeed those which Neddermeyer said, and

everyone said. But they needed experimental proof, and I did that. Then I thought that my usefulness at Los Alamos was over. Besides I just could not live under this atmosphere. It was a military atmosphere. Letters were opened and one was under constant surveillance and so forth. Maybe that's a rationalization. I came to Los Alamos in the summer of '43 and left in November 1943. The only reason I joined the project, like many of us, was the fear that the Germans might develop it and would be ahead of us. I had no real evidence for that. But I felt that this was not very likely to happen. If it had happened, I probably would have felt very badly, but it did not happen. So I left, somewhat to the annoyance of some of my friends, in particular Oppenheimer.

I left Los Alamos and joined then a group at Harvard at the Radio Research Laboratory, which worked on defense against radar. The Germans started to develop their radar, and our job was to find out what one could do against it. This work was almost entirely theoretical. I did some experiments with microwaves to find out the reflectivity of certain materials. It was pretty straightforward work and nothing really terribly profound. I don't know whether it was of great use. Van Vleck was the head

of that group—an old friend of mine. In fact, after the war, we wrote a paper together on the radiation of antennae.

Weiner:

By the time you got there it was well organized, though. Quite different from Los Alamos.

Bloch:

Oh, quite. It was well organized. We lived in the city, and we had a civilian life. I must say it was a great relief to me. I found it much more congenial there.

Weiner:

You were a newlywed, too.

Bloch:

Oh, well, no. We were three years married.

Weiner:

Had your wife gone to Los Alamos, too?

Bloch:

Oh, yes, with our firstborn children. My wife didn't mind Los Alamos. But of course since I was unhappy, she was perfectly glad to leave. It was a good time I had in Cambridge. And actually didn't realize that what I did there was going to be much more useful for my post-war work than what I had done and learned in Los Alamos. Of course, in 1945, in early '45, the end of the war was very much in sight. At that time I began to think about what one could do after the war. And Bill Hansen, whom I mentioned to you this morning, came quite often to Cambridge. He gave lectures at the Radiation Laboratory. He was a great expert. He was, of course, one of the early developers of the klystron and a great expert on microwaves. I also talked with Rabi quite a lot at that time. The Rabis lived only two blocks from us in Cambridge, so we saw each other quite often. I told him I would like to go back to the neutron work. I knew we could polarize iron—this much I knew already. And there was one problem that I felt. I think what limited our approach to a great extent was simply the measurement of the magnetic field. One had to measure the magnetic field and then one had to measure frequency. That's

all. The frequency was easy to measure, but the magnetic field is not so easy to measure, and Rabi knew this well from his molecular beam work; and we had all sorts of ideas that perhaps we could ship a permanent magnet to Columbia for calibration with molecular beams and then it would be shipped back to us. It sounded extravagant, as one would never know what happened to the magnet in shipment. At that time war work was not so urgent anymore—early '45. I began to think whether one could not do it in some other way: And then I found out that one doesn't really need molecular beams to study the nuclear magnetic resonances—Rabi did only molecular beams—but that one should be able to do it in condensed matter, not in a vacuum, in liquids in fact. I thought almost exclusively of water from the beginning.

So I had this idea then of what I called nuclear magnetic induction.

In fact, now the term nuclear magnetic resonance is applied almost exclusively to this field, because it has become practically much more important. But strictly speaking, I invented the phrase nuclear induction because I wanted to state that nuclear

magnetic resonance was not our invention. That had been done by Rabi and his people before. But the fact that the signal was received by induction, in the Faraday sense induction in a coil, is why I called it nuclear induction. However, the word didn't stick? It is now used in a very special sense. I called my first paper "Nuclear Induction."

Weiner:

You were consistent.

Bloch:

It is, of course, a technical point. Anyway, so I was all ready: I did my work mostly in the evening, my calculations, convincing myself that this should at least be possible; that the size of the signals were big enough. At that point I had learned something about radio techniques, and I knew what a receiver and an antenna was, and I knew what noise was. That was all-important in our game. That was of great importance to the work I was planning then. That's why I said before that I learned more, for me, useful things at the Radio Research Laboratory than in Los Alamos, because I knew very little about radio techniques before I joined this Harvard laboratory. I

may interject that that is probably a rather common experience. People felt that through their war work they came in contact with other fields of science, which they had not been acquainted with before. And I believe at least part of this sudden quick development of physics after the war was due to this phenomenon. It was certainly eminently true in my case, but I'm quite sure other people had the same experience. They had worked in too narrow a field, and now they were forced to go into other things, which they thought they would never use. They did so only because the war required it, and later it turned out it was very important.

Weiner:

I know that's true in the case of radio frequency techniques, because at the stage of development of nuclear physics, in order to produce higher energies you needed these kinds of techniques. I know that came out of it, but I don't know too many other instances. In your case the microwave technology again tied in. But I wonder if it was true for other aspects of war work. I know people learned something about nuclear reactions—that's true.

Bloch:

Well, you see, you mustn't forget, of course, in each field there were experts who may not have learned much, but people who were taken in from other groups who did not know anything about it and then learned it—I'm quite sure that there were many people in Los Alamos who knew very little about nuclear physics altogether, let alone fission, and learned it there. And then, of course, the technology and metallurgy of uranium; these were all things that people didn't know about and learned. How terribly important they were in the long run, I cannot say: But I think it started a general stirring up of interest that I think was very beneficial.

Weiner:

Would you say that this also involved people? In other words, you were speaking of coming in touch with new fields, but you also came in touch with people.

Bloch:

With new people. That's certainly true. Now, in my case this was not particularly true, because of course

my association with Bill Hansen was very old. In fact, the early work on the klystron—I was in on that and knew all about it. And we met each other frequently. He was down in Garden City with the Sperry Company. They worked on klystron problems at that time. But he came often to MIT and we met. I remember very well how we walked over the Charles River to a restaurant to have lunch and I told Bill that this was something I wanted to do when I came back to Stanford. We knew it would come pretty soon. He was very much interested and promised his collaboration. Even though I knew something about radio techniques, I needed somebody, and Bill Hansen was a great expert, and he immediately thought of ways of how that could be put into practice.

He also told me on the same occasion that he also had thought about something, and that was in essence what is now the linear accelerator. His early interest in microwaves came from the problem of accelerating electrons. That's how he started. But again the war, and of course the tremendous gain in power that people achieved in the meantime, made him feel that one could build actually a larger electron accelerator than this one we have here, the

300-foot accelerator. He died before it was finished, but it was very clearly his child. The big accelerator is only an offspring of that.

So then two really great projects were brewing for Stanford. When Bill and I went back we both had our hands full.

I came back here on a very modest scale. We had very little money at that time, by the way. The affluence of money came only somewhat later. Our whole first experiment on nuclear induction cost, I think, \$450, of which you had to spend I think \$300 on cathode-ray oscillography because even that didn't exist. We used an old demonstration magnet, a lecture demonstration magnet, which was not too good. Later on we realized that the best type of magnet was just barely good enough. We managed somehow. It's amazing how much one could do with little money if one needed to.

Weiner:

When your expectations aren't high, as far as getting funds, then you do what you can.

Bloch:

They were practically nil. There was one little thing. Bill had already gotten some money from the Sperry Company, and he had one machinist whom he put at our disposal. We had other machinists, but there was one man who was really then put on the work. Everything was handmade.

Weiner:

Where did the funds come from? The \$450?

Bloch:

Oh, that came from the University.

Weiner:

From the departmental budget?

Bloch:

The departmental budget; that much was allowed.

Weiner:

Let me ask a question getting back to the cyclotron. How long did you run it before you shut it down for the war?

Bloch:

Until '43. It must have been almost two years. The first year or at least half year was spent on this work I told you about—improving and studying polarization—and the rest spent on fission neutrons.

Weiner:

So it was involved in the war work.

Bloch:

Oh, yes. This work on the energy distribution from fission was done here.

Weiner:

It wasn't clear to me that it was the same machine, but then again you wouldn't go out and build another one. Whatever happened to that?

Bloch:

Oh, that was in use a long time. That finally went, I think, about six years ago. Meyerhof joined us after the war came and used it for a long time. I think in 1960 only, I believe, it was finally given as a present to a small university somewhere in St. Louis or

some such place. I think per dollar there is no high-energy machine in the world that has served better.

Weiner:

Your giving away small cyclotrons is like the old practice of giving away small telescopes. The observatories, as they graduated to bigger and bigger ones, would give them away. There's a trickle-down effect.

Bloch:

I don't know whether that cyclotron is still running. If so, it's probably just used for graduate students to get a little experience. It's really a toy.

Weiner:

You did some useful work with it.

Bloch:

Yes, we did.

Weiner:

You started, though, on the new work. You mentioned \$450. But actually what had to be done?

Bloch:

Well, of course, all the radio equipment had to be built. That we assigned to a graduate student, Packard, who is now with Varian Associates. He built the radio equipment according to the design of Bill Hansen. I myself was most involved with the magnet itself and its properties. We had another student, Manning, but he dropped out. So there was just the three of us—myself, Hansen and Packard. We went rather slowly at it. We got the first results early in January. I came back in August or September of '45. In January of '46 we had the first positive results. I heard just about at that time from Professor Stern in Berkeley that Purcell had a similar idea and had developed something. I didn't quite know what it was, but it was clear to me that it was in principle the same idea.

Weiner:

Had you known Purcell at all before?

Bloch:

No, I'd never met him before.

Weiner:

At none of these laboratories or meetings.

Bloch:

No, strangely enough, not. He was at the Radiation Laboratory, I believe, in Cambridge, but we never met. I met him later. Well, we both then presented our results at the spring meeting of the Physical Society in Cambridge, and that's where I met him the first time. We compared our notes, and we realized we were speaking an entirely different language but realized very quickly that it was basically the same thing.

Weiner:

You presented your results. This was one of your short papers.

Bloch:

Ours was only a letter to the editor. And so was Purcell's. It had appeared in the number just preceding ours. I think ours was January '46, and his must have been December '45.

Weiner:

You have one on October 1st and 15th, 1946.

Bloch:

That's a more extended one. This was a letter.

Weiner:

As a matter of fact, I have a copy here.

Bloch:

Is this my bibliography?

Weiner:

It's not a full bibliography. I have a '48 one.

Bloch:

Here: Physical Review, No. 70, page 474, 1946.

Then I wrote an extended paper. No, it's the earlier one: Physical Review, 69, 1946, page 127—a letter; that's what it was, a letter to the Physical Review.

And what you are referring to is, I think, my extended paper. That appeared later, and then was followed by a joint paper with Hansen.

Weiner:

You have one by yourself and Condit and Staub.

Bloch:

Yes, that is the one on the polarization I mentioned before, which is the last one we did before we went into the Manhattan work.

Weiner:

So the one with Staub in '46 was the publication of the results of the pre-war work?

Bloch:

Well, there is one on neutron polarization and ferromagnetic saturation. That must be the one—in 1942 and '43 with Hamermesh-Hamermesh and Staub and myself: "Neutron Polarization and Ferromagnetic Saturation." And '46 with Condit—I remember now—it was this way. The one with Hamermesh and Staub was to study the magnetic effects accompanying neutron polarization. We established an experimental law of how the polarization increased with the approach to saturation. Our magnet was not strong enough to go

higher, but we did know by extrapolation that if we could get a stronger magnet, we could do it. Now I remember: The 20% was not achieved until this paper with Condit after the war. But it was based on our earlier knowledge. Then we had money, and could build strong magnets. It's very interesting how one forgets.

Weiner:

Actually, you have to reread the papers to be sure. That was following up the polarization work, but then we were talking about the nuclear induction work.

Bloch:

Yes, after the war. Well, we did both. I mean I did both simultaneously. I worked with Staub and I worked with Hansen, although Hansen dropped out of it very soon because he turned all of his attention then to the accelerator work.

Weiner:

At this meeting in Cambridge you presented your results in a brief paper.

Bloch:

And so did Purcell.

Weiner:

Did anyone know to bring you two together or did you just find yourselves...?

Bloch:

Yes. We had known about this in the meantime. I knew of Purcell's work.

Weiner:

You say Stern had told you about it.

Bloch:

Stern told me only in a preliminary way. But then his paper appeared already in December, and I had read it? I don't know whether I wrote to him at that time. It wasn't necessary. So we certainly knew about each other in detail. I think Bill Hansen went to see him in Cambridge, I think in February before we met, and they discussed this.

Weiner:

This is '46 now.

Bloch:

'46. But I didn't meet Purcell until that meeting.

Weiner:

And subsequently has there been the need for collaboration?

Bloch:

Well, I think it was a very useful thing: Purcell and I, so to say, split up the field. We said: "You do this and I'll do that." But the Cambridge group was more interested in work on crystals, I think, perhaps largely because of Van Vleck's influence and interest, and they did some very fine work there.

Pound was there and Bloembergen was there, some very good people. Now, we specialized much more in liquids. I'm talking now about what happened after 1946. We did two major things, I think, here. One was after we had established the whole effect and studied it in detail on protons in water, simply in water. Then we proceeded to use it for precision

measurement of many, many nuclei. This was primarily Proctor, but that was later. We started already at that time. Proctor built special equipment. We had several machines going then in the basement. And that was one line, which we followed through, simply the precision measurement of every possible nucleus: Everyone posed some problem of its own because we always wanted to have them in liquids. It was a question of solutions in which they could be dissolved and so forth. But once that was done, it was usually very simple to measure the moments with accuracy and determine signs.

Weiner:

Other than people who were pursuing the same line of work, who was most interested from other fields in your results? Because it would have applications in solid state, in particles, in nuclear physics.

Bloch:

Yes. Well, I think the measurement of the magnetic moments, which I told you about, was of interest to the nuclear people, because at that time the shell model had started to develop and of course the magnetic moments of nuclei were a test of the shell

model. So I think that was the interest which the nuclear physics people took. As far as solid state, it was done at Harvard. They did some on crystals. I don't recall details now. Of course at that time I think ours were the only two really active groups in the field. Other people started, but it had not proceeded yet to the point where it could be used as a tool for many applications. There was just too much to be learned about the technique itself and what one could learn from it. Speaking about our own work, already in the very early work I could say I was always fascinated by the question of accuracy. That is to say, I don't want to become too technical, but if you don't mind: We reported already at the very first meeting in Cambridge about something called the relaxation time—that is, the time which it takes for these nuclei in water, for example, to realign themselves and establish equilibrium. We measured that and found it to be about two seconds. I think later measurements gave it somewhat more accurately at something like three seconds. But the amazing thing is that it was so long. Now, because of the indeterminacy relation, if you want, the relaxation time could have a direct connection with a frequency width. That is to say, if the relaxation time

is two seconds, and nothing else complicates matters, then one should be able to measure frequencies down to one-half of a cycle per second approximately. That was, of course, extraordinary because our frequencies at which we worked were about 30 megacycles; so it corresponded in accuracy of about one part in ten to the eighth. That, of course, fascinated me. But we never observed such sharp lines, because our magnets were not homogeneous enough. They were broadening simply because the magnetic field varies over the sample; and if it varies only by a part in a million and very probably more than that, it obscured already the effect.

Therefore, it was our goal then to go and see whether one could actually establish frequencies to such accuracies, get lines of such extraordinary sharpness, much sharper in proportion than any lines in spectroscopy one had had before.

That was the other main line, which we pursued, which turned out then to be practically a question of magnet design. It just simply meant that the pole faces had to be polished and very accurately aligned and so on and step-by-step...

Weiner:

This was done here.

Bloch:

That was done here, yes. This is what one calls now high-resolution work. I think I may claim that was altogether our project. I mean some people thought: "Oh, well, if you have it to a part in a million, why do you want it to a part in a hundred million?" But to me it was a sort of fascinating question. And, of course, my paper had dealt a great deal with the theory of relaxation time, and I really wanted to understand what was going on there. Then, out of this, later in 1960 or so, grew probably the most important application of all this.

There is perhaps one thing more I should tell you before. It must have been about 1950—in fact, my name may not even be on the paper— that there was a paper by Packard, Arnold and Dharmatti. It was a rather important paper. It dealt with the structure in alcohol.*

<I>(*Arnold, Dharmatti and Packard, Chemical effects on nuclear induction signals from organic

compounds, Letter in J. Chem. Phys., 19, 507 (April 1951).)</I>

But I want to tell you something else before that if you don't mind, because I want to finish up this question of the magnetic moment of the neutron, which, as you see, was always my companion since the very early days. I mentioned that in my Nobel lecture.

Weiner:

I remember the theme.

Bloch:

Because then in 1947, after we had established these high polarization effects with Condit, and the nuclear induction was working, it began to become a tool. In fact, one of the earliest applications which we realized right away was that it was the ideal magnetometer because if you put a proton sample, water let us say, into a magnetic field, then all you had to do is to measure the resonance frequency and you knew the field to a very great accuracy. But we did even better than that. Packard built a magnet stabilizer. That is to say, you can use the signal,

which comes out to readjust the field automatically electronically. And since we needed not only a very homogenous field but also a very constant field for the neutron experiment, we combined this. That is to say, we measured the field with great accuracy. We also used a nuclear induction stabilizer. Then we did the experiment for the neutron moment once more, and did it with very great accuracy.

Weiner:

In a sense you knew exactly what you had and you could maintain it at that same level.

Bloch:

Right, right. One could always hear. You had an earphone or radio equipment and could hear from the tone how much you were off.

Weiner:

You learned to recognize noise in your war work.

Bloch:

Oh, yes. Well, that was not noise; this is a control device. You simply beat the frequency, which you have against a constant frequency and you know you

have to stay on it. You slip a little bit off, and an audible tone comes out. You turn back and it goes "oohhmm," and then disappears. And then you know you're all right. That's just gadgeteering.

Weiner:

But the way one feeds back into the other is very interesting. Now, all of this occupied how much space here?

Bloch:

We were not in this building. We were down in the old building, and we were down in the basement. We had a laboratory, I'd say, about twice as big as this.

Weiner:

Twice as big as this large office you mean.

Bloch:

Maybe a little bigger.

Weiner:

It's still small-scale physics.

Bloch:

At that time it was. At that time it was normal physics. Nowadays it would be very small-scale. At times I think we had as many as three or four simultaneous magnets and probably six or eight people working in the laboratory, sometimes two together doing something.

Weiner:

And these people got their degrees with you, generally.

Bloch:

Yes.

Weiner:

Meanwhile, what was Rabi doing in terms of this? Was Rabi at all involved in this?

Bloch:

Well, Rabi, of course, had done his work earlier; and then his group continued. After the War Rabi was not so very active anymore. It was Kusch primarily. They did not use this method. In fact, in the

beginning Kusch thought this was cheating; that was not the right way to do it. Later on he persuaded himself that this method could even be used usefully in conjunction with molecular beams. Molecular beams have their own right of existence, and they did, of course, this wonderful work about measuring the magnetic moment of the electron, which then tied up with Schwinger's work. That was Kusch. So this group pursued their own interests, sticking to their molecular beams.

Weiner:

So the ones closest to you would have been the Harvard group then?

Bloch:

Oh, definitely, definitely. We had a constant exchange with them, yes. As a matter of fact, I know it was at a meeting that this business of pushing the line width down to a minimum was decided, a meeting together with Purcell. We felt that some sort of a standard moment should be used in conjunction with measurements of the Bureau of Standards. And for a standard, you know, you need very great accuracy. So it was at least partly that. That was at

least the rationale besides my theoretical interest—the drive toward the accurate standard developing in these high-resolution techniques. And then out of it came also, not quite by coincidence, something else connected with the work of [Erwin] Hahn. He came from Illinois, a very original man; and he had developed something called the spin echoes. That was his own version or really a variation, a very interesting variation of our game where, instead of applying the radio frequency at a constant waveform, he put it on pulses and observed what came out from that. He developed it very ingeniously. He also, when he came here, started to work on substances other than water and found some peculiar structures, which he interpreted as chemical effects. Well, we wanted to see those with our own eyes when he sort of predicted that one might see some kind of a structure. And then these three people—Dharmatti, Packard and Arnold, collaborators of mine—established for the first time in alcohol these three separate lines, which have become very famous in the meantime, for those three groups of hydrogen. That is to say, that the protons in the same molecule need not all have the same frequency; but they are only slightly different,

and one could not see that until one went to high resolution. But if their frequency was about at 30 megacycles their separation was about 100 cycles; that still didn't require the separation of half a cycle, but one had to come pretty close to it. Then the further we went with the resolutions, the finer structures and details showed up. I believe the alcohol spectrum now, any time you see it resolved, is something like 48 lines or so, and some of them indeed separated by just a few cycles.

Weiner:

So it wasn't just an idle pursuit.

Bloch:

It was not an idle pursuit.

Weiner:

The better you did, the more knowledge you had.

Bloch:

But it sounds like foresight. We didn't have that. The chemists think nowadays we developed this exclusively for their Purpose, but we had no idea of it when we started?

Weiner:

In other words, these applications of the structures, for example, only came after you had established that.

Bloch:

Yes. That did not really get going until the middle of the '50s. The earliest work was achieved about 1950 or so, but that was just laboratory work, just seeing three broad lines in alcohol. And then, of course, come all the refinements and so forth. It took several years and there were other tricks, which we also had to invent: the fact that if you spin the sample rather than keep it stationary, you narrow the lines by averaging all the fields. These are technical tricks but very important practices. But, of course, it could not be used as a practical tool until all these methods were developed. And then the last major work which I did in 1954 ... Well, there was the spinning. That was a contribution that was done in '54 or in '53 maybe. And then Arnold and Anderson, two students of mine, did the first basic work, you may say, on alcohol particularly. Anderson then started to

look at other molecules to measure the structure, to explain it, to understand all about it really.

Weiner:

But still there was that central theme of your—that you apparently started back in the '30s when you were concerned with the magnetic moment of the neutron—that you pursued in one form or another.

Bloch:

Except, of course, by that time in 1950 the magnetic moment as such was well known. It became a tool. I visited Copenhagen frequently after the war. At one point gave a talk in Copenhagen, and then afterwards we met with Bjerrum. Bjerrum was a chemist and a great friend of Niels Bohr, and I was there, and Bjerrum was interested in what I told him. And Bohr said to him: "You know, what these people do is really very clever. They put little spies into the molecules and send radio signals to them, and they have to radio back what they are seeing." I thought that was a very nice way of formulating it. That was exactly how they were used. It was not anymore the protons as such. But from the way they reacted you wanted to know in what kind of

environment they are, just like spies that you send out. That was a nice formulation.

Well, anyway, I think that was almost the end of my main contributions to nuclear magnetic resonance—in 1954 before I went to Geneva.

Weiner:

Now, all during this time other things were developing in other branches of physics, and yet you stayed in a field that you were doing major work in, and that field didn't grow very rapidly compared to other parts of physics? Or am I wrong in that? Relatively, it didn't grow as large, for example, as the whole rush into particle physics and higher-energy physics.

Bloch:

Well, you mustn't forget that all these fields have a relatively quiet initial period where you don't hear much about it. And those who work in the field realize the progress, which has been made. Then at some point when it gets ready, it booms out and spreads out. Particle physics also had its very slow

beginnings. Until not too long ago, all one knew of particle physics was only from cosmic rays.

Weiner:

The late '40s anyway.

Bloch:

Well, I mean until the big machines really started to work. So there were also many years in between when people said: "What's the use of these big machines? They never yet have taught us anything new." It may look a bit as if this all came overnight—it didn't. And although our field is of course not as fundamentally important as that of particle physics, I would not say that its development was untypical. It is not slow to take ten years for a field to develop, apart from the fact that of course before these refinements were done, it was already being used by solid-state physicists. So it had become useful, too, before that.

Weiner:

Do you think that since about 1950 it has emerged as a full-grown, strong, growing specialty or that...?

Bloch:

Yes.

Weiner:

Or rather...? You see, the other alternative is that because it's so successful, it's sort of taken up by other fields and used.

Bloch:

I would say mainly the latter, yes: It became a tool. That is to say [my own intention was to use it] to measure magnetic moments, and that job was simply done by that time. And then it was taken up by other fields, partly in physics and partly not in physics. As I say, chemistry has become a very important user of the technique. But it's true that the art as such was practically finished.

Weiner:

Is that why you turned your attention somewhat to other things after that? For example, the theory of superconductivity.

Bloch:

Well, that came after I came back from Geneva, but I may say— and this is, I think, as far as you want to go today—that [explains] the fact that I considered to take the position at CERN at all, I think I can say psychologically it was due to the feeling that my necessity of staying with nuclear induction was not so great anymore. At least my own heydays were over.

Weiner:

That's interesting, because in this case the completion of the work, apparently the satisfactory completion of it, coincided with the recognition—in this case the Nobel Prize.

Bloch:

That came earlier. It came in '52. I still continued after that—until '54.

Weiner:

I understand that. By that time, you really had carried through this program of research that you had established for yourself many years earlier.

Bloch:

Well, I don't think we would have received the Nobel Prize if we hadn't done that. Not only did we have to develop the technique, we also had to show that something could be done with it.

Weiner:

But what I mean is: there are two things. One is setting yourself a goal and accomplishing it, and the other thing is that your colleagues recognize this.

Bloch:

Oh, I don't think it needed the Nobel Prize. My colleagues realized that before. That was only the publicity for the while.

Weiner:

So you felt that when you had completed it and it was good and it was done and people knew about it...

Bloch:

And I was quite confident that other people would take it up, and that was indeed what happened.

Weiner:

This puts you in the early '50s.

Bloch:

It is true that nevertheless I still did take some equipment to Geneva. Arnold packed it before I went to Geneva and actually did go with me to Geneva, because I didn't want to be entirely separated from nuclear induction work. They set up a laboratory at Geneva at the University. What little my time allowed me then, I still came and was interested in this.

Weiner:

Let me ask you about the prize. When you had done the work, had it occurred to you that this might be of Nobel Prize caliber?

Bloch:

Well, I don't know. I think we were quite aware of the importance of it. What gets a Nobel Prize and what doesn't, is not so sure to say. I had some inklings a few weeks before I received the prize but not before that. And then you know there is always

the usual gossip among physicists to which one better pay no attention to what they say: "I'm sure you'll get the Nobel Prize next year" and so on. No, I think quite largely it was a pleasant surprise. I must say in all modesty that it didn't come entirely out of the blue sky.

Weiner:

What was your reaction when you did get the word?

Bloch:

Well, I woke up early in the morning. They called me from Associated Press at 4 o'clock in the morning, I think, and I got up and I was so sleepy. When they told me that, my reaction was "Nonsense, I must be dreaming." But then very soon the telegrams started coming in. Well, as I say, I was not quite without warning. In fact, the Stanford Press had heard something about it, and a man from the information office came to me the day before and told me he had heard such rumors and said to me that I had better hold myself ready for the next day. "Well," I said, "I don't know what's involved." My wife and I sat in the evening before we went to bed, and I said to her, "You know, of course, it may all be

a false rumor. But even to think that it is possible is a very nice thought."

Weiner:

Yes. I'm sure that your colleagues here were overjoyed, along with you.

Bloch:

Oh, yes.

Weiner:

I think you were about to suggest that we talk about CERN.

Bloch:

That's right. Well, now, the first idea bout CERN came up in the fall of '53. I received a letter—I believe it was from Bohr—telling me about the importance of this new laboratory and asking me whether I would consider the directorship. I went through great struggles at that time, first feeling: "This is sheer nonsense. I shouldn't go into this kind of thing. I'm not made for administrative work." Then thinking that perhaps it wasn't altogether administrative. Well, finally in the spring of '54,

upon the insistence of Bohr, I went to Copenhagen and talked it over with him. He urged me very strongly at that time to take the directorship of the laboratory: He felt that it was important that an active physicist should head the laboratory, and he believed and gave me the impression that as director still my main function would be scientific. And although many of my friends warned me and said, "Don't believe it—it's not going to be that simple," nevertheless I felt at that point that I should try it. I was not sure that this was really the kind of work I wanted, but I felt I wanted to try it. I think I came to a decision some time in the spring of '54 when it was offered to me, and I made it a condition then that under no conditions would I stay more than two years. I had my doubts about the job right from the beginning. But anyhow I did go there then in '54. Heisenberg also was very eager. I think it was mainly Heisenberg and Bohr who wanted very much that I should take that job.

Weiner:

Well, they were on the council. Bohr was head of the theoretical side.

Bloch:

Yes, they were on the council. Heisenberg I think represented Germany on the council.

Weiner:

But October '54 is when the council announced your appointment.

Bloch:

That's when it was official.

Weiner:

That's when they made it official. When did you move there?

Bloch:

I think I went there in early October. It was probably the first council meeting.

Weiner:

Actually you made a public statement then, too, right after that, so you must have been on the scene.

Bloch:

Yes, yes. Oh, yes. Certainly. All the details were settled before that. I was in Paris I think after I was in Copenhagen. I was in Geneva. I met Amaldi there, and then I went to Paris where the council was meeting. Then at that point, I think it was that I said that if they offered it to me I would try it.

Weiner:

What was the situation in European physics at the time after the war?

Bloch:

Well, of course, they had fallen very much behind in the war in two very important aspects. The first was electronics, because radar techniques were far more developed here and of course in England, too. England had a head start on that, but it was then taken up with greater intensity here. And the second thing was clearly atomic energy. On both these things the Europeans were badly behind. I think it was Rabi who first suggested to them that they should get going on their own and have this large laboratory built in Geneva. It started officially when

I went there—I was the first director—but all the groundwork had been done by Amaldi.

Weiner:

I have this report that Amaldi gave, and he covers four years, from 1950 to 1954, showing the different stages, mostly administrative.

Bloch:

Right? But actually when I joined it there was already some work going on, on a very small scale. It was at the airport in Geneva.

Weiner:

Did it catch on in terms of being regarded as a prestigious place in which to work?

Bloch:

Well, of course, in the beginning it was not so much yet. That is, during the time I was there it was very much in its initial stages. We had, in fact, quite some difficulties recruiting people. People did not want to go to Geneva very much in the beginning, because you know how it is in Europe. They have their positions, their association with the university; they

don't easily give that up. We got two theoreticians from France. That's something I insisted upon: And there was a very good experimental group under Adams, but they were mostly engineers. They were concerned with design and construction of the large machine. When I was there, there was practically no equipment. There was a little work going on. The cyclotron was beginning to be built. And my time, unfortunately, at that time was indeed almost all taken up with administrative work.

Weiner:

What sorts of problems seemed to be the most difficult?

Bloch:

Well, I think the personnel problems were probably the ones that concerned me most, and then, of course, I was the connecting link between the staff and the council. I had to present to the council the wishes of the laboratory and vice versa. We had these council meetings, and they had to be prepared. There were also minor things. I had a British civil servant, and he was in charge of the purely technical administrative work, and I had great help from him.

But there were questions about personnel insurance, and I mean all the problems that come up are new problems. And in spite of the fact that I was not considered an expert, after all, being the director I had to acquaint myself with these things. I must confess I was rather unhappy. That is to say it became quite clear to me after a relatively short time that that was not the kind of thing—that my scientific interests were practically unused or were used to a very limited extent. Perhaps it could be that the people were very nice to me out of respect, because of my prestige. So maybe in that respect it helped. But I decided already in the spring of '55 that I was going to go to the end of the year and find a successor, but go no further than that. It caused in the beginning some consternation, but I think it was forgiven. So I left. I went back in '55. In retrospect I must say it was an interesting, enjoyable experience, but I'm certainly glad I did not carry it further.

Weiner:

You felt you weren't able to contribute anything or very much as a physicist except in terms of your general knowledge and maturity and wisdom. But in terms of a director responsible for policy, the policy

that was needed at that time didn't involve very much physics.

Bloch:

Very little. I mean the building of a machine was the main goal and that was already going.

Weiner:

Was this ever in dispute, by the way—the fact that this should be basically a high-energy physics laboratory?

Bloch:

No, that's what it was meant for from the very beginning.

Weiner:

And it could have gone in other ways?

Bloch:

No, no, the proton-synchrotron was already the goal. That finally started to function in 1960, I believe.

Weiner:

The thing that I would like to dig into with other people who were involved in the early policy discussions, is how that decision was reached.

Bloch:

That you must read in Amaldi's report. By the time I came that decision was already reached.

Weiner:

Amaldi gives very little on that, but Kowarski is writing a history of the early stages.

Bloch:

By the time I became director that was already decided. In fact, the group under Adams was already very active.

Weiner:

I have a press release that was issued on February 25th, 1955. "Professor F. Bloch has informed the council of his desire to resign his appointment to take effect from 31st August." So in other words,

you got there in October. It was less than five months when it became clear to you.

Bloch:

Yes. In fact, it was clear to me before that. Because between the official announcement and the decision of acceptance by the leading people, there was also a lapse of time.

Weiner:

That's true, because in the same release they mention your successor.

Bloch:

That had already been decided.

Weiner:

So they must have had time to do that.

Bloch:

No, no, no. It didn't take me very long. I felt I could not afford sticking my head into the sand. I realized from the moment I personally decided I didn't want to go on, that until I could really leave it would take several months, many months anyway.

Weiner:

Have you maintained contact in a policy-making or advisory position in any way?

Bloch:

No, I visit CERN whenever I am in Geneva, but I have not taken any role.

Weiner:

We have a couple of minutes. Let me ask a general question. And that is: in summing up and thinking about the work you've done through the years, which would you say has given you the most personal satisfaction?

Bloch:

Well, that is a difficult question to answer because there are several types of satisfaction. That is, for the immediate satisfaction, one of the greatest joys that a scientist can have is when a good idea hits him. That is, when headaches, which have been brewing in his mind for some time, come together to a solution: That happened to me a few times. But those are moments almost. Those are moments of elation,

which might last for a day or a week or two, or something like that.

Weiner:

Which instances?

Bloch:

There was a paper with Nordsieck where we stewed and worried for a long time, and suddenly I realized: "???" Ah, this is the crux of the matter. That gave me great pleasure. Well, I'm sure that when I first realized this idea of the nuclear magnetic resonance—just on paper it came, in checking and rechecking and saying, "Yes, by God, I haven't made a mistake. I'm not fooling myself. It should really be possible." That was also a moment of great joy.

Then, of course, there are also long-range satisfactions—that is to say, the development of a certain line, like this resonance work after the war, which extended over many years: It was not a period of constant happiness. I mean there were ups and downs, but by and large it was a time of great satisfaction.

Weiner:

How about in the other sense of the importance of the work as judged by your colleagues—that is, the long-range importance? If you had to identify a single piece or discovery, what would you select?

Bloch:

Well, I would say probably two. The one goes way back to my thesis. That was my work on conduction in metals. That certainly has had a great impact. And the second was, I think, the discovery of induction.

Weiner:

Nuclear induction.

Bloch:

Yes. These, I think, are the two contributions, which have had the greatest impact.

Weiner:

Of course I ask the question in terms of long-range. Would the same answer be true of immediate impact on the field, as well, for both of them?

Bloch:

Well, there's always a certain starting time, you see—from the time that a new discovery is made until its full importance can be objectively evaluated takes a certain time. I think one has a certain feeling of instinct right away whether something is at all important or hardly important at all. That's easy to say. But it's not easy to say whether something which may sound terribly important today may not be completely old-fashioned in three years or whether it will go on, and I must say I have had good luck with these two things. I mean the work based, I may say, on my early work on electron conduction is still going on. It's very important in solid-state physics. And nuclear induction has become a tool which is made use of widely.

Weiner:

That's quite different from discoveries as they occur in experimental physics. If you discover a new particle, you know, that's something that remains important.

Bloch:

Not necessarily.

Weiner:

Well, maybe recently things have changed. But I know that the neutron has. I mean a particle in that sense, a real particle.

Bloch:

Well, of course, I may say that my discoveries have never had the glare of the novelty of, let us say, the discovery of the neutron—I mean something totally unexpected almost. None of them were of that type. There was always some background that was growing in that direction. This supreme joy of hitting something really terribly important that nobody ever suspected before I have not had. Very few people have had that.

Weiner:

But you've had the parallel to it in cracking a tough problem.

Bloch:

Well, cracking a tough problem and then in some minor way realizing something which was expected before, I mean like, let us say, the fact that electrons can indeed run through a metal despite all the ions being present—that is also a very nice insight.

Weiner:

And you knew that, you had predicted it, even though there was a year or so before there was any experimental verification of that. I'm referring to the idea in '36, your paper in '36.

Bloch:

Yes. I was certainly pleased. I felt it was a very original idea. Nobody had thought before of combining ferromagnetism and neutrons and getting something useful out of it. This whole idea of magnetic scattering was a novel idea. What would grow out of it? No, I don't think I foresaw that at all.

Weiner:

I think we've covered a lot of ground here, a lot of things I probably will want to think through and

come back better prepared on specific technical things. But for the things that we said we'd do, I think we've done very well. I think you've done very well.

Interview Session – 3

Hoddeson:

This is Lillian Hoddeson and I'm interviewing Professor Felix Bloch in his office at Stanford University on December 15. Since you have already been interviewed in some detail by Kuhn in 1964 and Weiner in 1968, it will not be necessary to discuss your entire life starting from 1905 when you were born in Zurich, because the transcripts of those earlier interviews are available for qualified scholars at the Center for History of Physics. So we'll have the luxury of focusing on a few key issues relating to your work in solid state physics between 1928 and about 1933 or '34.

Bloch:

All right.

Hoddeson:

I'd like to actually begin approximately two years before the completion of your doctorate in Leipzig, with a point that you mentioned in your interview

with Kuhn and I suppose also in your memoir, concerning Heitler and London.

Bloch:

Yes.

Hoddeson:

You said you got to know them in 1926 when they came to Zurich.

Bloch:

Did you read my article on “Reminiscences of Quantum Mechanics”? I mention something there.

Hoddeson:

You mentioned that you took walks with them.

Bloch:

Yes, right.

Hoddeson:

And that you discussed the covalent bond theory.

Bloch:

I don't know that I discussed it with them. I knew they worked on that. We talked about all kinds of things, mostly about quantum mechanics. Maybe they spoke about that to me. I really don't remember. I couldn't say that I was engaged in that. I mean, I knew their work very well and it impressed me, as I said in my article which I sent to Mott, that in their model also there were (crosstalk)

Hoddeson:

Yes, yes, I see, because this theme comes up again and again then in your work later.

Bloch:

Which theme?

Hoddeson:

The electron topping from —

Bloch:

Yes, I made a, of course, I made a whole point of, yes (crosstalk knocks out a lot of this...)

Hoddeson:

OK, we won't make too much about the impression.

Bloch:

Well, of course, I knew them very well, and it was for me very important to talk to people who were much more advanced than I was.

Hoddeson:

Right.

Bloch:

At that time. But I would not claim any credit on the theory of covalent bonds, no.

Hoddeson:

OK. Now, in 1927, Debye suggested you move on to Leipzig, where Heisenberg was, because all the people you might have wanted to work with in Zurich, Weil, Debye, Schrodinger, were leaving just about this time, or had left.

Bloch:

Well, actually, there was a slight mistake, Peierls told me afterwards that my memory lapsed. I think Weil had already accepted the post at... but he did not leave already in '27. He left a few years later. Peierls wrote to me because he was assistant of Pauli, after I was, during that time, he is still went to a lecture of Weil's, so that is not quite correct. Weil did not leave in '27 but it was known that he was going to leave, and he left shortly afterwards. Furthermore I would not have worked with Weil because I'm not a mathematician.

Hoddeson:

Why did you choose Heisenberg rather than say Sommerfeld?

Bloch:

On Debye's recommendation. Debye told me that Heisenberg was coming to Leipzig, and said, "If I were you, I would work with Heisenberg."

Bloch:

That was decisive. I trusted Debye's judgment in this.

Hoddeson:

I see. All right, in your PHYSICS TODAY article you describe your first impressions in Leipzig very well, also in the Mott memoir, but I wonder if you could expand a little bit more and give us just a little bit more detail about the research environment in Leipzig? As I see it, the quantum theory of solids, which I'm particularly interested in, emerged in Munich, and then within a very few years was dispersed first to Leipzig, Zurich, then to many other countries.

Bloch:

Yes...Yes. And it's quite possible that Heisenberg, who of course also was in contact with Sommerfeld, got this idea that one should — or was interested in that because of Sommerfeld's work. That's very possible.

Hoddeson:

Right. Right. Now, Bethe went into quite a bit of detail about Munich.

Hoddeson:

He told us about the rooms and about the courses, for example, and I'm wondering whether you could perhaps tell us a little bit about Leipzig as you saw it at that time ? For example, the courses that were of interest. Heisenberg gave a solid state course.

Bloch:

No, No, no, no, no. He did not give a solid state course. No, He gave the (crosstalk) poh, sure. No, he gave the introductory course on — well, he gave a course on quantum mechanics. I remember I sat in on it. I did not attend particular lectures at that time. I mean, I worked for my thesis. But I did go to a lecture of his, and I remember I was very impressed about the way, how he introduced quantum mechanics, told us how he had introduced it, by deriving it from the Krammers-Heisenberg dispersion formula. You know, his way of getting at it was quite different from Schrodinger's or Debye's

(DeBroglie's?) and I remember that very well, how very logically he showed us that the matrix formulation followed almost necessarily from the Krammers-Heisenberg dispersion formula. So that is one of my very vivid memories I have. But then of course it was a very good department. Wenzel ? was already there. You know that.

Hoddeson:

He was there already.

Bloch:

Oh yes. A year later he became Schrodinger's successor at Zurich, but for the first year I was there, he was also there. And Hundt was there, and Debye was there and Heisenberg was there, so you see that was a collection of really first rate people. It was a large department.

Hoddeson:

Now, were the seminars in the same style that one finds here in this country?

Bloch:

Very informal. Very informal.

Hoddeson:

Did graduate students participate?

Bloch:

Well, of course, graduate students — that concept didn't exist in Europe. Everyone was a student. But, yes, there were a few. The number was very small, I'd say about six people. And Heisenberg was always very nice and ordered cakes. So, he paid for the cakes, for the seminar. It was very informal but on a very high level.

Hoddeson:

I see. You say there were about six people. Were these six people —

Bloch:

These were students and assistants. I don't quite remember them. Now, for example, Teller, who came later, was not there, yet? Right. Weisacker was not there either yet. We're talking about the first years. Very small. I think I was the only student of Heisenberg. And this was an extremely, shall I say, relation of almost friendship. I mean, he and I

would take our skis and go over weekend skiing on the southern border of Germany, and we went together to lunch and so forth.

Hoddeson:

Did you talk about physics when you went skiing?

Bloch:

Oh yes. Oh yes, sure. Well, you see, physics and everyday life were not separate in our lives. It was all the same.

Hoddeson:

I see. And these seminars, did they meet on a regular basis once a week, or were they?

Bloch:

I'm not quite sure how often. Yes, they were fairly regular. Whether it was once a week, or — I think so, yes. My memory isn't quite good. I think so, yes. Now, for example, Heisenberg wanted me, shortly after I came, he wanted me to report on something I had read on, oh, this five dimensional Geometry fellow. I don't know whether you know that. Oh, there was an attempt by Klein and other people to

bring electricity and gravitation together, in a five dimensional geometry. It was interesting, at that time. Heisenberg wanted to know about it. I hadn't looked at that before, so I gave for example a seminar on that. It had nothing whatsoever to do either with Heisenberg or what I was doing. It was all just general information. A paper he didn't have time to read, so he said, "Would you tell us about it?"

Hoddeson:

It was a combination of seminar and journal club?

Bloch:

You might call it that way, yes.

Hoddeson:

But Heisenberg selecting the particular articles that?

Bloch:

Yes. Yes. I think I told him that I had read these papers and he said, "Why don't you tell us about it at the seminar?" or something like that.

Hoddeson:

I see, and these seminars would run an hour, hour and a half?

Bloch:

Yes, about that. Then we sat together or we started to play ping pong. It was all very informal, very very nice.

Hoddeson:

I see. Now, I did come across some notes of a course that Heisenberg at least wrote in note form. I don't know whether the course was ever held. In these papers that were collected by the Kuhn Project in the early sixties, I think it was... I'm not sure. I don't have it written down.

Bloch:

Well, that's possible. I rather doubt that he gave it during that year that I was there.

Hoddeson:

Not during that year. It looked as though it might have been dated '29, '30.

Bloch:

‘29, ‘30, yes, I wasn’t there then.

Hoddeson:

Only because Peierls’ processes are mentioned, but there’s nothing about bands or diamagnetism or anything like that, so that would date it in that year.

Bloch:

I wish I’d seen that. Do copies of that exist?

Hoddeson:

Well, I made a very bad copy. It’s quite thick. I can send it to you.

Bloch:

No, no, that’s too much.

Hoddeson:

No, no, it’s — a Xerox copy — I’ll send you a Xerox, well, I only have a Xerox copy.

Bloch:

Yes, but that's yours. And you could have one made? I mean, could I keep it?

Hoddeson:

I'll send one to you. No, there's no problem with that. They're not expensive.

Bloch:

That would be very nice. I didn't know that, you see.

Hoddeson:

Well, you might see it, and might have a comment to make, That would be of interest to us, so — OK, well, yes, the interaction between theorists and experimentalists in Leipzig at that time. Later Wilson talks in his memoir about Gooden coming to tell them about semiconductors, and I was wondering whether in general, there was some contact.

Bloch:

Well, we knew them of course very well. I mean, for example, Debye's assistant, Sach, he was a friend of mine. We came together from Zurich, so I knew very well what he was doing. I knew he was

working on these electrolytes and that kind of stuff that Debye was interested in. I think so at least.

Well, we knew what they were doing, but I do not recall experiments done in this particular field, at least at Leipzig. Could be that I don't remember. Now that you mention a name it sounds familiar to me, but —

Hoddeson:

There wasn't that much. What about the work that was being done in some of the other places, for example Greenhausen in Gottingen.

Bloch:

Greenhausen in Gottingen. Well, of course, I think I probably met him before, but it was just — of course he wrote to me for example, when I got finally the key to the fifth — he said, ah, now that makes sense. So from time to time you know he wrote sort of interpolation formulae that contained both. I think I knew probably Owell and there was work done at Seaman's, and... and so on. I knew there was a lot of work going on in solid state physics, but my contacts were not terribly good.

Hoddeson:

They weren't, on a personal level?

Bloch:

Well, personal, yes, but not scientific particularly.
Well, you know, there was a certain attitude among the theorists at that time, so to say, we don't really need the experimentalists, we know better.

Hoddeson:

There was, really?

Bloch:

Well, I'm making a little bit of a joke.

Hoddeson:

Yes, sure.

Bloch:

The separation between theory and experiment was much greater in Europe than it is here.

Hoddeson:

Than in America.

Bloch:

Yes. Yes. I knew them, I knew them personally. But you didn't just go and ask an experimenter some curve, and then you say, aha, now I have to explain this curve. It was the other way around. You made your theory, and then you came to him, and if his experiments agreed with it, fine, if they didn't, you said he made a mistake. I mean, I'm a little facetious, you'll remember.

Hoddeson:

Yes, Yes. That fits with what I've heard from other people. About the visitors passing through, I gather there were a great many.

Bloch:

Yes. Yes. And I pointed that out to Peierls. Wait a minute, where did Peierls? Oh no, that was another article— yes, Peierls wrote an obituary for Heisenberg. Do you know that?

Hoddeson:

Yes.

Bloch:

He mentioned several visitors there, and I pointed out a few more to him. For example —

Hoddeson:

— who are they now? I don't remember the obituary. You don't have to go back and look it up.

Bloch:

Oh, there were an awful lot.

Hoddeson:

Hausman? Slater, of course.

Bloch:

Slater? Not in my time. People kept on dropping in. Now, wait a minute, are we still talking about '27, '28?

Hoddeson:

'27, '28.

Bloch:

During that time there were not so many yet.

Hoddeson:

Not so many.

Bloch:

No, No. It was later. When I was Heisenberg's assistant there were many more.

Hoddeson:

Well, since we're on visitors and we may get off on other subjects, let's talk about the visitors in general.

Bloch:

Well, of course, a very important visitor for me was Hausman, because he had worked with Sommerfeld on the same problem.

Hoddeson:

Right. Right. Right.

Bloch:

But of course he made a perturbation calculation. You know.

Hoddeson:

Yes, I know, also he —

Bloch:

—but I think us basic idea was quite right. The interaction between electrons and sound waves, he treated very well.

Hoddeson:

Right, and he also had the idea of the infinite conductivity.

Bloch:

Well, about the periodic potential, that I don't know, and I don't think so. He just improved on Sommerfeld's theory by saying, OK, there are — there's not just the Fermi gas but there are also sound waves, and the electrons scatter on them. I'm not too positive. One can look that up in the literature. I don't think that he realized that one can have a strong interaction of the ions, if the ions are addressed. I mean, he sort of took it for granted that the electrons somehow travel through a lattice, if it is uniform.

Hoddeson:

I think he had the idea that the deviations in periodicity had something to do with the resistance.

Bloch:

Of course. That is correct. That means, deviation of periodicity means sound waves.

Hoddeson:

Yes.

Bloch:

But he treated them I think more or less as a continuum, and, well, and I think — I would say, his is really an approximation. It's an approximation of weak, weak coupling between the electrons and the ions. And within that framework, it was identical with mine, and actually, after I wrote my T to the fifth paper, he had trouble with it —

Hoddeson:

Yes, I know.

Bloch:

But he came and said, “Yes, that’s right, I got that too.” That’s obvious.

Hoddeson:

Well, he got a T to the third.

Bloch:

Well, in the beginning, we both got it wrong.

Hoddeson:

You both got very different errors he corrected and said (crosstalk) then you agreed—

Bloch:

Yes, but the — by the time I showed the correction, he said, “That’s right,” and that didn’t surprise me at all, because, I mean, the coefficients would not be the same, but his approach was quite the same as mine was. That the resistance was due to collisions with sound waves, or as you say, phonons nowadays.

Hoddeson:

Let me just mention some of the people, other visitors. Just to find out whether you interacted with them, not necessarily in this year but perhaps later.

Bloch:

Heller, of course. Well, Keller was not a visitor. Heller was also assistant in Leipzig. He was a student of Wundt We worked together for a year.

Hoddeson:

Oh, he was assistant to Wundt?

Bloch:

Yes. We corrected together.

Hoddeson:

This is the year?

Bloch:

That was, I think, '32. '30, '31. Yes.

Hoddeson:

OK, Slater I guess just in '29.

Bloch:

Well, I am not sure that I remember Slater. I mean, I knew him later, of course, here.

Hoddeson:

Well, there's a reference in your paper on the first ferromagnetism paper.

Bloch:

Oh yes, the determinant formulation.

Hoddeson:

He saw a preprint of his paper apparently that he shows you —

Bloch:

Oh, that is quite possible — oh, he showed it to me?
(crosstalk here)

Hoddeson:

— in Leipzig —

Bloch:

That's possible.

Hoddeson:

At least there's that much of an interaction but maybe not a great deal —

Bloch:

Yes — well, no, but you see, all these people stayed in Leipzig maybe a week or so, and during that time, we talked to each other. Now that you say it, it's probably true, that I was very grateful for that to Slater, because it was a very nice formulation.

Hoddeson:

OK, what about Landau?

Bloch:

Landau I met later. Well, let me think. I met Landau during the time that I was in Copenhagen. No, no, just a minute, let me correct that. Just a moment... I met Landau, well, I'm sure I met him the first time in Copenhagen. Now, whether that was during the time I was — it could have been during the time that I was Pauli's assistant and just visited Copenhagen. You know, we went there quite often. So I'm not quite sure. I know for sure that I met him some time

between 1927 and '28 and '30. In Copenhagen. And then, I spent a year in Holland. That was in 1929-30, and he visited me in Holland, and there we were —

Hoddeson:

In Utrecht?

Bloch:

In Haarlem, yes. And then we talked about all kinds — we were very good friends. At that point he told me that I must visit him in Russia, which I did later. And we talked, oh yes, we talked about physics, again like everything, went to museums, he liked paintings.

Hoddeson:

You were really very close to him.

Bloch:

Oh yes. Oh yes.

Hoddeson:

Because there's quite a lot of contact with Landau and the kinds of ideas that Landau was working on in the later papers we will get to.

Bloch:

Yes. I wouldn't be at all surprised if as didn't come up casually, so, in our conversation, you know.

Hoddeson:

What about Nordheim?

Bloch:

Now, Nordheim, where did I meet Nordheim first? I think it was on a visit in Gottingen. I think he was Born's assistant, if I remember correctly. Anyhow he was in Gottingen, and we went to Gottingen occasionally for seminar. I think it was even during the time that I was Heisenberg's student, that I went to Gottingen, where I met Max Born the first time, and Nordheim, I'm pretty sure.

Hoddeson:

Frankl?

Bloch:

Frankl, which Frankl do you mean?

Hoddeson:

Josef Frankl.

Bloch:

The Russian. Oh, ja, I thought you were talking about Gottingen. Yes, I believe I met him later on my visit to Russia.

Hoddeson:

Later.

Bloch:

Well, I told you, Landau invited me and I went to Russia.

Hoddeson:

At that time 1932, '33?

Bloch:

'31.

Hoddeson:

‘31, OK. All right, Peierls I won’t ask you about because there was so much interaction along the way. Briant?

Bloch:

Briant, yes, I, that was somewhat later. After I left Germany in 1933, I spent a few weeks in Paris.

Hoddeson:

At the Institute.

Bloch:

Right. In fact, I lived there with Lengema. Yes, I knew him very well, I knew his daughter very well there and then I stayed at their house. And Briant — well, I’m not quite sure if I had known — and then Briant, we went out and talked. That was in Paris.

Hoddeson:

Because I noticed —

Bloch:

— then I later met him when he was in his country, during the war.

Hoddeson:

I don't quite know how to fit his work in.

Bloch:

Oh, the Briant zones.

Hoddeson:

Yes, there's a lot of, it doesn't —

Bloch:

Well, I'm sorry to say, my memory's not too good on that. Do you remember when he wrote his paper on the zones?

Hoddeson:

1930. Then he wrote his second book, French textbook, I can't remember if it was '30 or '31, but the German one which was much more widely used was 1931.

Bloch:

Well, I rather think that I met him personally and again we became good friends, after that, I somehow do not associate Briant very much in my memory until 1933.

Hoddeson:

Was he the kind of person who interacted much?

Bloch:

Oh, very much. Very much so. Oh yes definitely. A very lively person.

Hoddeson:

I see. There's not much information about Briand.

Bloch:

Well, you know, he had a good deal of engineering background. In fact, he worked for the radio stations of France for a while, and he had a very very sound grasp on reality. You see, his knowledge of waves, in which he was a great expert, derived partly from his work with radio waves.

Hoddeson:

I see.

Bloch:

He knew about that — until the war, when we worked together, in fact we published a paper on that.

Hoddeson:

That's very interesting.

Bloch:

Briand was a very very fine man to talk to.

Hoddeson:

Somehow in his book, there are places, he references you all the time and talks about your thesis and your work in general. Peierls, he seems to have had a running argument within his footnotes.

Bloch:

Briand?

Hoddeson:

Briand with Peierls, not the other way around. But he doesn't like the processes, and he doesn't mention Peierls' work on the band gaps.

Bloch:

I see.

Hoddeson:

He may have mentioned it, but he doesn't —

Bloch:

— I don't know about that —

Hoddeson:

— give him as much credit —

Bloch:

— well, I don't know, frankly, I don't know, it may be that maybe they didn't hit it together personally so well, maybe — well, I got along with Briand very well.

Hoddeson:

I don't have good information on that. It's just a feeling I had. Maybe it isn't even right.

Bloch:

Well, it may of course have been that perhaps he knew me better personally and therefore gave me more credit. I don't know. I know Briand and I got along together very well, I mean, our thoughts running really parallel. Peierls' way of thinking was perhaps more strange to Briand. Maybe it was. Actually I spoke French and Peierls didn't.

Hoddeson:

Oh, that might have had something... well, let's look at the work now. The first problem that Heisenberg suggested to you, I guess it was in the fall of 1927, concerned ferromagnetism.

Bloch:

Right.

Hoddeson:

That is, to compute the Weiss field of 1 to 1907.

Bloch:

Well, that he already knew.

Hoddeson:

He already knew, OK.

Bloch:

He didn't work out the details, but he said, "Look, there are these strong forces in the Weiss field, there must be something else, there must be an electric force, there must be exchange," and then he just sort of sketched it to me briefly. That's it. He knew it.

Hoddeson:

Did you feel that this was really opening up a whole new era in magnetism?

Bloch:

Oh yes, of course — well, no, it is not such a great prospectus as you have now, but I said, "Oh ja." You see, everything, all of a sudden people explain things. I said, "Aha, that is the explanation of ferromagnetism." I wasn't terribly, I didn't think of ferromagnetism as the most important thing in the

world. Neither did Heisenberg. It was a nice little byproduct.

Hoddeson:

It was a way of cleaning up the problems that — quantum mechanics...

Bloch:

— well, yes, that's right, and Heisenberg evidently felt that there are open things in solid state and the Weiss field was one. And he probably just thought about it, that must be it, and mentioned to me, "There's no point in my going into it," although he didn't do it, his mathematics wasn't very good.

Hoddeson:

Then you did something about it. But you didn't expect to go into it all necessarily yourself? at that time?

Bloch:

Not at that point, no. I mean, I felt, I'm not going to compete with Heisenberg.

Hoddeson:

Did he talk about the problem with you while he was finishing up the details?

Bloch:

Well, I think he showed me the manuscript.

Hoddeson:

But you didn't criticize it?

Bloch:

Oh, well, no. I would not have done better at that time either. That came later.

Hoddeson:

Your main work was your thesis, which, you discussed it at length in several articles.

Bloch:

Oh yes. Oh yes. Oh yes. There I was in constant contact. Now, in one way Mott and Peierls are not fair, in their obituary. They say that Heisenberg suggested to me to study electrons in the periodic potential. That's not true.

Hoddeson:

What is true?

Bloch:

What is true—he said, “Look, there’s all that work of Sommerfeld and Pauli on the free electrons, and I think it would be interesting to look into that.” I mean, it was sort of understood that electrons, the... is not a vacuum in which electrons simply run. That was about it.

Hoddeson:

I see, and then you decided to just take the ions at that point, not to worry about the electron interactions with each other?

Bloch:

First — well, as I wrote in my article, the first thing was, why is it, how come they say that electrons don’t run free through a metal? And as I say, that was the — now, you ask, if it is periodic, they practically run through free. That was I think the main step. I mean, as I said, I said, “That’s it,” because you see, somehow, people must have been

waiting for that. I was not the only one who said it was absurd to think that electrons just run freely through a metal, and so, the moment I said, “Well, of course the ions are there, but they can run anyway,” then most people felt I got so much applause after that, that I was quite surprised, you know, but it shows that people had been waiting for that. I was not the only one who thought that was crazy. And so.

Hoddeson:

Sure. When did you learn about the work of Wismer and Rosenfeld?

Bloch:

I think only (crosstalk)

Hoddeson:

You mention it in a footnote.

Bloch:

Yes, that’s right. I think, well, maybe somebody pointed out to me, it might even be Heisenberg, somebody else, that there was a paper on periodic potential before, then I read it. But as you just

pointed out, they found that out but they didn't attempt to follow it further. You read that paper?

Hoddeson:

Yes, some time ago, not recently.

Bloch:

I read it 50 years ago so you know it better than I.

Hoddeson:

When did you learn about Floquet's theorem?

Bloch:

Oh, that I don't know.

Hoddeson:

But after?

Bloch:

Oh yes, after.

Hoddeson:

Much after, I see, so you, independently —

Bloch:

But you know, Floquet, that came from celestial mechanics. There was a question of periodic perturbation of planetary motion. Well, it was, mathematically it was the same problem. I think his periodicity was entirely out in space.

Hoddeson:

I haven't looked at his work.

Bloch:

Well, I think it was in reference to celestial mechanics.

Hoddeson:

If I have a chance it would be fun to look at that. Some time. See what he does do. You know, it's probably different from what people say he does.

Bloch:

No, no, I think, frankly I don't quite know myself, exactly what Floquet's theorem is, whether it has this band structure, or whether it shows these modulated motions, I do not know.

Hoddeson:

I don't either.

Bloch:

But I know it has to do with the effect of a periodic potential (perturbation?).

Hoddeson:

Did you have any idea at this time of other bands besides the first one?

Bloch:

I think so. I think so.

Hoddeson:

Bands in general?

Bloch:

Well, yes, because you see, I started, I studied the opposite limit. I said, "Suppose electrons are in first approximation simply bound to each state." Then I said, "But then you must make a combination." But then it was clear that since the atom has excited

states, to each excited state there would belong a band, and this (crosstalk)

Hoddeson:

— you felt that was clear to you at that time?

Bloch:

Oh, absolutely.

Hoddeson:

In 1928.

Bloch:

Oh yes, sure.

Hoddeson:

But it doesn't appear in —

Bloch:

— no, no, no, I wasn't interested in higher bands.

Hoddeson:

No, no, but you had the idea.

Bloch:

Oh yes. Oh yes. There can be no doubt about it, because when you start from the isolated atom, then of course, I said something about the ground state band. It was clear that the upper states would be equally spread.

Hoddeson:

Did you have any idea at that time that the idea of explaining the difference between a metal and an insulator?

Bloch:

No — well, yes, but you see, there I made mistake. It's even mentioned in my thesis. I thought that the difference is simply a question of binding. It was stupid of me, not to realize, at that point — Peierls said to me later, "It seems rather obvious that a filled shell must be..." I said, "Yes, but I missed it." And Wilson pointed that out to me.

Hoddeson:

Was that the first time you came across that idea, in 1931, when Wilson?

Bloch:

I didn't come across that idea. No, I had the wrong idea, and then Wilson correctly described some of these Products. Stubborn, I said, "I don't believe it." I'd been working quite wrong. I said, "In that case, a divalent metal would have to be an insulator, "But then he pointed out to me that other bands can cross in.

Bloch:

You see, if it were a single band, then with two electrons per electron, would be filled. And then I said, "How can that be? There are metals who give off two electrons per atom, they're still metals, they're not insulators."

Hoddeson:

Right.

Bloch:

But then he was very smart, and said, "Well, you have to think about it a little bit." But then he came back and said, "Yes, but you see, the bands may cross."

Hoddeson:

What about the idea of gaps between the bands?

Bloch:

Well, it is also quite obvious, when you start from the opposite limit. You see, the excited states are widely separated, and if they jump only occasionally, each is slightly splits on the [?] cross diagrams.

Hoddeson:

So the fact was — that was obvious?

Bloch:

You see, the fact — oh yes — well, obvious, look, you have to be careful. The fact that there were bands, several bands, and there were gaps was to me totally trivial. I mean, that, I said, so what? I understand that. But what I missed was a, that this makes a difference, the difference between an Emulator and a metal — incredibly stupid. And also, and then, how semiconductors, all that sort of thing, that by excitation, you can create electrons. I just didn't think of it. But as I say, the existence of bands

and the gaps between the bands, that was, I wasn't moved by that, not at all.

Hoddeson:

I see. Did you read Bethe's thesis at that time, by the way, on the Davidson and Wermer experiment?

Bloch:

I don't remember it. I probably did. I read pretty much everything at that time.

Hoddeson:

Because I read that recently, I just brought it along, and he actually has the band gap in a peculiar way. He doesn't say, because what he's concerned with is electrons,

Bloch:

— scattering —

Hoddeson:

— scattering, yes, and he has regions —

Bloch:

Yes, where they cannot go in.

Hoddeson:

Where they are totally reflected.

Bloch:

Yes. Well, you know, this is of course something that already Avard knew, on the scattering of X-rays.

Hoddeson:

It certainly is in here. He calls them regions.

Bloch:

Does he quote Avard's theory? Dynamical theory of —

Hoddeson:

— he has reference to —

Bloch:

— ja, ja, ja —

Hoddeson:

— Avard —

Bloch:

— yes, because, you see, that was of course like Haaston too. They considered the analogy between X-rays and electron waves. I mean, we know already something about rays going through — of course, mainly from the X-rays — and so therefore I think it is quite clear, he wasn't particularly proud of that either, because that is probably in Avard's paper. On the scattering of X-rays.

Hoddeson:

I see. I'll have to check that. But in a way, that corresponds to the gap.

Bloch:

Ja, I wouldn't be surprised if that same thing appeared in Avard's paper.

Hoddeson:

Avard is a good deal earlier. OK, I'm wondering whether the picture of the Fermi energy surface was apparent to you at that time.

Bloch:

No, not particularly in those terms. I made my life immediately very simple, because then I took it as a sphere. I made a quadrilateral approximation on that. I realized that it is not a sphere, but it wasn't terribly important to me. I mean, all these things later by Schoenberg and so on, on Fermi surfaces, were not

—

Hoddeson:

— not of interest to you.

Bloch:

No.

Hoddeson:

But in a way, you have some expressions for the energy which then are plotted out in the Sommerfeld—Bethe article —

Bloch:

That may be the cosine dependence, the sum of cosines, yes. Yes, it's true, that gives you a kind of a Fermi state, but I wasn't interested in that. I wanted to calculate the resistance. It was too complicated for me, so I approximated it by the quadratic minimum. That then is a sphere.

Hoddeson:

Right. Now, I gather you did talk to Peierls about the hole effect while he was working, on it, or...

Bloch:

— probably. Probably.

Hoddeson:

And there he also works with almost filled bands. The first bands.

Bloch:

That is because he has these holes, the matrix hole effect? I thought that was ingenious, but I didn't think of that. You see, by that time, of course, in Peierls' work on the hole effect, the idea of filled bands and holes in bands was already much more obvious.

Hoddeson:

A question on the hole. Peierls doesn't mention it very clearly, but the idea is really in his hole effect paper —

Bloch:

That holes have actually positive charges.

Hoddeson:

Yes, although he doesn't come out and say that.

Bloch:

No.

Hoddeson:

You say that in an article in 1931 which I probably have here, in the — You do have a picture of holes here.

Bloch:

Oh, that was a talk I have.

Hoddeson:

This is a talk... I think it's here...

Bloch:

Is that mine?

Hoddeson:

This is yours. The question is, where is?

Bloch:

Here, you have something -- no, no.

Hoddeson:

But that's not -- I'm looking for the hole. I was sure the hole was in here.

Bloch:

Ja, well, I probably had that. That was much later, wasn't it?

Hoddeson:

This is 1930-'31?

Bloch:

Ja, well, I must have mentioned Peierls' work there too. That was not my idea.

Hoddeson:

OK, anyway, the physical picture of the hole is here, and also in a paper by Heisenberg.

Bloch:

Yes. Now, look, this is an interesting thing — because Heisenberg had for a while the idea, and I think there are two, that the protons — not the positive electrons, but that the protons could be holes, in an electron sea. And I remember Heisenberg once gave a talk in Copenhagen on this idea. But —

Hoddeson:

These are the holes in quantum electrodynamics.

Bloch:

Yes.

Hoddeson:

Not the holes in solids.

Bloch:

Had nothing to do with — well, it had to do with — but I think it's quite true that the idea that a hole could be a positive electron came already from Dirac's hole theory. I mean, that was not a terribly original thought of Peierls. He probably didn't mention it because he said, "That's well known." I think so.

Hoddeson:

So you think there was a connection between the hole in quantum electrodynamics and the hole in semiconductors?

Bloch:

I think so. I think so.

Hoddeson:

In Heisenberg's mind certainly.

Bloch:

Yes.

Hoddeson:

Maybe, we don't know whether it was in anybody else's mind.

Bloch:

Well, now, I must confess, at this point, my chronology is a little bit mixed up.

Hoddeson:

Well, the hole appears in Dirac's paper first, clearly in 1931. I think so, and the analogy —

Bloch:

Oh, Peierls' was before that.

Hoddeson:

1929.

Bloch:

Ah, then it may even be that he had learned from Peierls.

Hoddeson:

If he read that. But Peierls doesn't describe the hole very clearly. He has a mathematical equation.

Bloch:

Ja, well, he showed that you get the negative hole effect, this is an electron missing. That's all.

Hoddeson:

Yes. But he doesn't talk about it. He doesn't use the word. The word "hole"...He just talks about it —

Bloch:

Ja, well, that may very well be.

Hoddeson:

It doesn't seem to be clearly described until 1931 in, through —

Bloch:

— probably through (crosstalk)

Hoddeson:

— Heisenberg —

Bloch:

Now, then Dirac came. You see, I think this is rather a leapfrog thing. Heisenberg or Peierls showed the negative hole effect, the presence of holes, and then Dirac must have known something about solid state physics, — well, he also thought it was — he also thought at first they were protons. Dirac.

Hoddeson:

Dirac thought, in quantum electrodynamics, that they were protons.

Bloch:

Yes. But it was clear, these were positrons. And then by that time, the idea that a hole in a filled sea acts like a positive charge was known, and that probably, Peierls realized that his hole effect could be explained by simply assuming that they were electrons with positive charge. I think so. I'm guessing here. I'm sorry, I —

Hoddeson:

It would be fun to work.

Bloch:

I'm not a reliable historical source on that.

Hoddeson:

It would be fun to work that out.

Bloch:

There was so much interplay, you know, between all the physicists at that time, that as soon as somebody had an idea, another one took it up and put it in a different form, used it somewhere else, and so forth.

Hoddeson:

Right. Oh, I had another question about your thesis: the application of group theory was only an elegant way of writing it up?

Bloch:

Yes. Yes, I called it —

Hoddeson:

You found the Bloch waves using 48 analysis (crosstalk)

Bloch:

Yes, sure, sure, sure. I mean, as I say, I followed the fashion at that time. Maybe it is, it probably is more general there. I think it's the only time in my life that I made use of group theory. And it wasn't even necessary.

Hoddeson:

OK, let's go on to Zurich, where you spent the next year, 1928 —

Bloch:

Pauli, no, I'm sorry, Heisenberg was so impressed by my group theory thing that he said, "Oh, you have a way of determining group characters." I just barely knew what group characters are! So he looked at it and said, "Oh, that's wonderful, that's a new method in group theory." Of course it wasn't new at all, groups are very well known.

Hoddeson:

I see, Heisenberg was not up on group theory?

Bloch:

Well, Heisenberg, he took the same casual attitude as I took, on group theory, it might be used once in a while.

Hoddeson:

Did Slater's method really play this role that it's reputed to have played, in slaying —

Bloch:

— ja, well, you see, — oh, I see. Slater. That was group very well afterwards.

Hoddeson:

Afterwards, well, that's what — (crosstalk all in here)

Bloch:

— slay — yes, I think, we all were very relieved that one had a much more familiar way of expressing the content, than all this general highbrow group theoretical arguments. Well, Ehrenfest put it very well, he talked about group pest. But he was wrong

and so was I. We underestimated the power of group theory at that time.

Hoddeson:

You, in your Kuhn interview you described going from Leipzig to Zurich as going from optimism to pessimism. And mentioned that this partly had to do with the people involved, and the politics.

Bloch:

— well, and the politics.

Hoddeson:

Well, it was mainly Pauli?

Bloch:

Well, you see, the difference between optimism and pessimism is the difference between Heisenberg and Pauli. Heisenberg was a very optimistic man and Pauli was a very pessimistic man, by nature. That's why Pauli was so effective as a critic. Whereas Heisenberg's critical abilities were not very great. Although Pauli made mistakes. I mean, he fell for my wrong theory of superconductivity: there, he was not critical where he should have been.

Hoddeson:

In your piece for the Mott volume, you portray Heisenberg's feeling about solid state... You describe in the Mott volume article Heisenberg's feeling about solid state in 1928 as "a field to which quantum mechanics could fruitfully be applied."

Bloch:

Ja.

Hoddeson:

Now, was this also true of Pauli?

Bloch:

Pauli— Weisskopf told, Pauli said to him once, "I don't like solid state physics, although I started it."

Hoddeson:

Yes. And yet he always had his hand in somehow, though, in some way.

Bloch:

Well, yes, his application of Fermi statistics was —

Hoddeson:

— was absolutely — the first —

Bloch:

— very important, and then he —

Hoddeson:

And then he was —

Bloch:

That's what he meant when he said he started it.

Hoddeson:

Right, but he was always interested in superconductivity.

Bloch:

Well, I think I've described it properly. He just felt, let's get done with this, let's explain it all and then go to more important things.

Hoddeson:

I see.

Bloch:

Yes, he was interested in it. He was interested in it. Much much later, for example, this was not at Zurich, I was already in America, he pointed out to me a paper by which he said was important, and he was right, it was.

Hoddeson:

Hm

Bloch:

Well, I mean, Pauli, after all, he was a physicist, and as much he could not entirely ignore interest in problems in solid state physics, but he didn't really have his heart in it.

Hoddeson:

You also say in the Mott volume that you started to work on superconductivity before you went to Pauli.

Bloch:

Well, that's saying a little bit too much — I certainly thought about it, yes.

Hoddeson:

And how far had you gotten?

Bloch:

Not beyond the first idea, to say, that must be a minimum in the energy.

Hoddeson:

— and that the electron —

Bloch:

You see, that was not a trivial thought, because, one might have said, why do currents persist? Maybe it has some strong selection rules, that, and then I said, that cannot be, because the superconductors, they're not even particularly pure, and all that. So therefore, there must be insensitive to... and that can only be an... and that was the only idea I had. I seem to remember that Landau told me something once similar, probably, some time — I said, "That must be it."

Hoddeson:

This idea that you and Landau had about the same time and independently, I guess, of the energy minimum —

Bloch:

I'm not sure but I do not think that Landau ever published anything about it. Neither did I. We never published.

Hoddeson:

No, I learned this from the Sommerfeld Bethe, where he discusses this idea that you and Landau had. That is not so bad.

Bloch:

No, it's not bad at all, it's — proves, in fact, only how difficult it is to get that, we didn't realize. But, well, I think, I probably told it to Bethe. I have a feeling that it came up in talks with Landau. Either I mentioned it to him or vice versa.

Hoddeson:

But, when it might have come up?

Bloch:

Probably in Holland. Probably in Holland. No, no, excuse me, no, before that. Before that. Oh, no, wait a minute, it might have been quite different. It might have been that I had this idea, but heard from Landau only when I met him, which was later, that he had the idea too, because I had this idea before I went to Pauli. I remember. In fact, maybe I mentioned it to Pauli and he said, “Yes, OK, go ahead and finish that work.

Hoddeson:

Do you have old notebooks in which you might have scribbled something?

Bloch:

Oh no.

Hoddeson:

You don’t keep those things. Too bad. One could trace things like that. Well, it’s not always possible.

Bloch:

See, I didn't predict at that time that this would be the real later.

Hoddeson:

OK, that's very interesting. At what time did you state the theorem that Laundau quotes you, that all theories of super conductivity can be disproved?

Bloch:

I don't know. It's one of those jokes, you know. Laundau's formulation? That might well be.

Hoddeson:

In London. In London's book.

Bloch:

Oh, I see.

Hoddeson:

I was just wondering whether that dated back to the time when you were with Pauli, or?

Bloch:

Yes, probably after that — when I realized why I failed, then I made that general statement.

Hoddeson:

Were you up on other people's theories of superconductivity?

Bloch:

Well, not up, but I know that Heisenberg for example tried it at one point. He had some idea of condensation in space, as he called it. Yes. He may not have published on it. I think, the way he put it, his theory would have failed for the same reason for which mine failed. You see, in the long range, all of us, that we missed. That was the point.

Hoddeson:

What about other people working on it as well, Frankl, Kronig, Elssasser?

Bloch:

Yes, that is possible. I don't remember much about that. I mean, of course, superconductivity was a

problem to be thought about. Well, it was common knowledge.

Hoddeson:

You wrote your 1939 paper on, “The susceptibility and resistance of metals in magnetic fields” while you were in Zurich?

Bloch:

In Zurich, yes.

Hoddeson:

Yes, and you thank Pauli for stimulating this work, and for clarifying remarks.

Bloch:

Ja.

Hoddeson:

I was wondering how he felt about this problem. Here you were correcting him, in a way.

Bloch:

Well, he felt good about that, because it was a sort of extrapolation of his own work on the paramagnetism ferromagnetism.

Hoddeson:

Paramagnetism.

Bloch:

He liked that. You know, one always likes one's children to do it, something else —

Hoddeson:

The other paper you wrote in this period was on the ferromagnetism possibility of paramagnetism by conduction of electrons.

Bloch:

Ja, and, well, — right — when electrons are not low? cost? current?

Hoddeson:

You say this is an outgrowth of Pauli's paramagnetism paper, and I didn't understand that.

Bloch:

Well, I think it was — again, I'm not too sure. It was insofar as one talks about magnetic properties of conduction of electrons, I mean, insofar, it is of course an outgrowth. I mean, Pauli was the one who pointed out in his paper, on paramagnetism, I mean, he was the properties of conduction electrons. So that, to that extent it was. In the meantime, Heisenberg had his paper on ferromagnetism, so then there was the question, could one have only paramagnetism, with free electrons, or couldn't one also have ferromagnetism? I think in that sense, it grew out of it. I mean, it was rather obvious, considering Pauli's paper and Heisenberg's paper, this problem was not too far fetched.

Hoddeson:

Now, a key achievement in this paper is your, the first calculation of the interaction energy of the electron gas.

Bloch:

Yes, I think that's probably true.

Hoddeson:

You have a calculation, and, it's one level less than Wigner's later calculation in 1934.

Bloch:

Yes.

Hoddeson:

In fact, you're quoted in a footnote in the Wigner paper. He cites you for this. So this is, I didn't realize, this is where that was done first.

Bloch:

Yes. But again I had no such general ideas in mind. I mean, it's — the simple question was, after all, is it necessary for ferromagnetism, to think that the electrons are localized like Heisenberg did? And then I realized, no.

Hoddeson:

They have to be very far apart.

Bloch:

Yes, well, I gave some conditions on it.

Hoddeson:

That's right.

Bloch:

Whether it would or whether it would not (crosstalk)

Hoddeson:

— yes, yes, yes. Conditions, I guess it's this...

Bloch:

I'd like to talk more. You remind me about all kinds of things.

Hoddeson:

Well, tell me. It's OK. We don't have to stick to my questions necessarily. They may not be the best ones.

Bloch:

No. Go ahead.

Hoddeson:

But then, once you find you get this result, then it's clear that the Heitler-London method is probably better, isn't that true?

Bloch:

I don't know that I particularly thought that way. Did I say that?

Hoddeson:

No.

Bloch:

Oh, no, no, no.

Hoddeson:

That was a comment I took from, I guess Slater mentioned that in his autobiography. You know, I guess it's this paper in which you thank Slater somewhere for the —

Bloch:

— I don't think anybody read my papers as carefully as you do.

Hoddeson:

Yes, here it is. Well, this doesn't necessarily mean you talked to Slater about it. On page 568, you thank him for a preprint.

Bloch:

Well, that was later. That was not a lapse of [?] research. This paper was written in Zurich.

Hoddeson:

This paper was written in Zurich?

Bloch:

Yes, and I don't think I — it might well be that I met Slater only in Zurich. It could well be.

Hoddeson:

I see. Could be, yes. In fact I think Slater did mention that it was in Zurich, so, I was wrong before. But then in discussing this paper in his book, Slater goes on to say that, then he got a preprint of your paper, and while he was in Leipzig after that —

he must have stopped in Zurich and then gone to Leipzig —

Bloch:

— while I was at Zurich, he was at Leipzig, that could well be, yes.

Hoddeson:

And he was at that time exploring the relationship of the Heitler-London method to the Hundt-Mulligan method, and he noticed that your paper, your approach and Heisenberg's to ferromagnetism —

Bloch:

— corresponded to —

Hoddeson:

— corresponded to those two approaches. And then he went on with this idea, and did more on ferromagnetism.

Bloch:

That's right.

Hoddeson:

Stimulated in that way. Have you studied Frankl's theory of ferromagnetism?

Bloch:

Apparently not, because really at this point it doesn't ring a bell at all. I may have forgotten.

Hoddeson:

Let's move on to the Netherlands, for 1929 to '30. You were there on a Lorenz Foundation Fellowship.

Bloch:

Right, in Utrecht. And later I had something else, kind of an assistantship in Haarlem.

Hoddeson:

With Fokker.

Bloch:

With Fokker.

Hoddeson:

I see, and how was this arranged, with — the fellowship, the assistantship?

Bloch:

I don't remember. While I was still in Zurich, I think. I don't remember whether I wrote to Krammers, could I come to you?, or they wrote to me, will you come to me? I had also an offer as an assistant to Max Born at that time, and Pauli said, "Don't go." "He's a mathematician."

Hoddeson:

I see. I gather you got along very well with Krammers.

Bloch:

Oh yes, very well.

Hoddeson:

Where was the meeting of minds?

Bloch:

First of all, I think I met him before already, and then, well, he was just very pleased that I would come, on a fellowship, and decide to spend it in Utrecht.

Hoddeson:

I see. Now, what was he working on then, at that time?

Bloch:

Oh, all kinds of things. Among other things, on the comintization (?) of the spinning top. I know he had a student by the name of Ickmann. He was not particularly working on these things during this time. He was just a wonderful person. He was an excellent musician. He played cello very well, talked about poetry, and of course physics. Again, it was the same thing.

Hoddeson:

It was here that you found this error in your thesis and solved the problems.

Bloch:

Yes. But that was not because of Krammers.

Hoddeson:

He just happened to find it? Or no?

Bloch:

Well, you see, this was a great time for me, because I had no obligation. All I had to do was think and so on. I thought, and I realized there was something fishy in my thesis. But I realized simply that the homogenous equation has a solution, if one...and then I realized, how one has to be careful.

Hoddeson:

Then you had your big paper on the Heisenberg theory at low temperatures.

Bloch:

That's right. It was a spin base.

Hoddeson:

Yes, yes, and —

Bloch:

Well, that was another thing. Then I said, well, now, let me think of it -- Heisenberg, that's very complicated, maybe at low temperatures -- well, the first important thing was, to realize that the absolute minimum of the state is, all spins parallel. There's only one state.

Hoddeson:

Yes, right.

Bloch:

Then I said, well, OK, that's rigorous. Now, suppose we flip one spin around, or two, or three. Then I didn't go too far, because then it became dangerous.

Hoddeson:

Were you surprised when you found the spin waves? You don't call them spin waves yet in this paper.

Bloch:

No. No, no.

Hoddeson:

But very soon after that, you do call them spin waves.

Bloch:

Yes.

Hoddeson:

I guess it was at a talk you gave in Leipzig.

Bloch:

Yes. I didn't call them spin waves till then?

Hoddeson:

Maybe I missed it.

Bloch:

I thought so, I...

Hoddeson:

Well, maybe you do.

Bloch:

I think so, spin waves, didn't I say that already in the

Hoddeson:

I think this is where it appears first in the paper.

Bloch:

That is this paper, yes. Well, I don't know. I mean, that they were spin waves is obvious. Whether I used the word or not.

Hoddeson:

Is there a story behind how you found the spin waves?

Bloch:

No, not particularly. You see, of course, in a way it was very much the same thing that I was familiar with from the hopping of electrons in a metal. The way I started my thesis. I said, well, if electrons can hop, spins can also hop.

Hoddeson:

I see. But that's (crosstalk) – a somewhat different idea.

Bloch:

Well, it was related. It was related. First of all, I was not impressed by Heisenberg's mathematics, though the fundamental idea was clearly right. And then I said, maybe one can do something more rigorous. And then I said, well, if at all, maybe one can do it at low temperatures. And I really don't know more about that. That's why I wrote the – wrote to Mott and said, "I don't think I can write a great deal about ferromagnetism, because I don't really, I mean, it was just all kinds -- you see, it's this way, yes. When I came to Krammers, I had before been Pauli's assistant and so forth, and then also there was a time when I was quite free. I could do what I want. And then, you sort of pick up old things which you have in the back of your mind, where you had doubts before, that maybe I can do something — just pick them out. One was my doubts about the integral equation. And the other was that I felt, well, yes, Heisenberg is basically right, but his mathematics

isn't very reliable. And then, the only way to do anything more rigorous to me, seemed to me, at low temperatures, and that's true. If you had been able to go further, I would even have written a paper on phase conditions.

Hoddeson:

Well, you do almost, a few years later. I guess you discussed this with Krammers?

Bloch:

Oh yes, of course.

Hoddeson:

Was he happy?

Bloch:

Yes, indeed. I'm sure I wrote something about it. Don't I have at the end?

Hoddeson:

Yes, you thank him at the end.

Bloch:

Oh sure. Oh yes, of course. Oh yes. I told Krammers.

Hoddeson:

Later Krammers wrote a paper with Heller in 1934, attempting to develop the spin wave theory on a phenomenological basis.

Bloch:

Who did that?

Hoddeson:

Krammers.

Bloch:

Ja, well, but that was not the super-exchange. That was something else I talked with him about later.

Hoddeson:

I see. You make a point about dropping the spin orbit coupling here. A big point of it, and I was wondering whether that was something that was

particularly in the air at the time. The fact that you bother to make this big point about dropping it.

Bloch:

I don't remember that. That was of course much more important later, when I talked about the main walls, you know, Well, —

Hoddeson:

OK, this you did in Utrecht, and then you gave a talk in Leipzig, but while you were in Holland —

Bloch:

That was a conference, I believe, yes. I went to Leipzig for a conference.

Hoddeson:

A conference organized by Debye.

Bloch:

Right. Right. Right.

Hoddeson:

Leipzig, ? I gather this was the third one, so it sounds as if there was a regular series of conferences.

Bloch:

Yes. Yes.

Hoddeson:

Organized by Debye, is that correct?

Bloch:

Yes, that is correct.

Hoddeson:

And there was one in 1930?

Bloch:

Yes. Yes.

Hoddeson:

Mott was there?

Bloch:

Yes, I think that was the only one I was — Yes, this was on solid state. Mott and Mark, oh yes.

Hoddeson:

Your paper, I just have a little more Xeroxed on your paper.

Bloch:

I don't think that was particularly new. That was more a summary.

Hoddeson:

Yes, it was a summary, but here you actually give the physical picture of the spin wave, and you call them spin waves.

Bloch:

Ja. Ja.

Hoddeson:

In this paper.

Bloch:

Ja, ja, because you see, that was sort of a more popular talk, so —

Hoddeson:

And you talk a lot about this interaction between the electrons, the need to take these into account.

Bloch:

Yes. Well, in a general talk, one tries to line out the essential problems.

Hoddeson:

Were other people worrying about the electron-electron interaction besides you? I mean, it's not very much in the literature, that I see.

Bloch:

But I spoke about, not in context with superconductivity, it was ferromagnetism.

Hoddeson:

Yes, but later when you wrote your review, you say, the big review —

Bloch:

Oh, the book article.

Hoddeson:

Yes, that, at the very end, everything looks like it's going to be solved, except superconductivity and the transition to ferromagnetism, and you say, well, but we still haven't taken the electron-electron interaction into account, and maybe the —

Bloch:

— that's right. Well, at that time, I had no doubt realized already that for superconductivity, you would have to take it into account.

Hoddeson:

Do you know when you realized that it would have to be taken into account for superconductivity? Was that quite early or did that come out of your work with ferromagnetism? You started on superconductivity before ferromagnetism, right?

Bloch:

Yes. Yes, yes, well, started, yes, sure. That's right. I'm sorry, what is your question?

Hoddeson:

The question is, when you realized that the electron-electron interaction would be important for superconductivity?

Bloch:

Well, as I say, I think I had that idea already sort of in my mind when I came to Pauli. It must have been at Leipzig. Because, well, I mean, as I told you, when Pauli said, or maybe I mentioned to Pauli something and he said, "Yes, yes — yes, go ahead and experiment on superconductivity, "I'm sure that the electron-electron, because that was the main thing that was missing in my paper. And then also, as I said, here, there was this analogy between ferromagnetism, between Curie temperature and... temperature. I had that feeling, there must be something in common, or between ferromagnetism and persistent currents, and both persisted. The magnetism persisted and the currents persisted. So

how could that be? And I felt that must be a common cause.

Hoddeson:

Then you went back to Leipzig, 1930-31.

Bloch:

Right.

Hoddeson:

As Heisenberg's assistant.

Bloch:

Right.

Hoddeson:

And then you, in the current review, you say at this point you wrote a long and learned paper on ferromagnetism.

Bloch:

Yes, sure, that was for

Hoddeson:

Now, which one is that?

Bloch:

Of course, this was still, the... was coming.

Hoddeson:

That's the... but that's later. (crosstalk)

Bloch:

No, it wasn't later, I worked on it in the summer, in the summer of '31. Yes. No, you see, as a matter of fact, Heisenberg said to me I should... myself, or become a — you know the procedure — and so I wrote something for them. I said, "Will that do?" and he said, "Yes." But then it was probably published later. I went to Copenhagen then, you see.

Hoddeson:

Right. It was published in '32.

Bloch:

And my former — in '32, right. It was written in '31.

Hoddeson:

Yes, you submitted it in September, '31.

Bloch:

There you are, see?

Hoddeson:

Right. You also wrote a paper with G --

Bloch:

Gentia, oh yes. Yes.

Hoddeson:

No, who is he?

Bloch:

Well, he's an Italian. His father was a Senator, very important man in Italy, and he spent a year in Leipzig, and he was a nice fellow, but helpless and didn't quite know what to do, and also again I spoke a little Italian, so took him under my protection, and then we worked together on this.

Hoddeson:

On this :

Bloch:

Right. That was of course very important, because that came in then here. That was, I think, ja, right.

Hoddeson:

OK. I think we should move on to the big paper because we're running out of —

Bloch:

By the way, there was a man by the name of Becker. You know Becker. And we had discussions on that quite frequently, on ferromagnetism, and this question of in ferromagnets, I think he made me pay attention to that, and in Heisenberg's spin, it would be entirely isotropic, and then we said, that must be spin orbit interaction, and I think that stimulated then my paper with Gentia. But that paper played a very important role. We discussed it a great deal.

Hoddeson:

I see. Let's move on to this paper —

Bloch:

We had meetings. You know, Eckles in Berlin and we were in Leipzig, and we met in a little village in between, just to talk about ferromagnetism. Heisner was there too.

Hoddeson:

In that way you learned about the experiments by Stictus and Tonks?

Bloch:

Probably. Very probably. Yes.

Hoddeson:

They seem to be the takeoff place for this.

Bloch:

Ja, ja, right. Problem for the walls.

Hoddeson:

For the walls?

Bloch:

Yes, yes. Yes, that's very possible. I think that's probably true.

Hoddeson:

You would have been in touch with?

Bloch:

Ja, ja, Becker was, wasn't that...He was much more in contact with industrial research than I was. And I think he pointed that out to me. I'm sure. Yes.

Hoddeson:

They use a very elegant Dirac transformation theory. In this paper, to get your equation for the spin wave function. There are references to Dirac. Here's one, and well, it's quite — (crosstalk) quite an elegant, you know —

Bloch:

Well, you see.

Hoddeson:

It is a very learned paper.

Bloch:

Well, one had to, sort of. When you wanted to become a teacher at the university, you had to show that you knew something. So, that's probably why I put that in.

Hoddeson:

I see. Let me just quickly ask a few of the things that struck me.

Bloch:

You know, there are all kinds of funny things also about the partition function, sort of little jokes I played with then did you look this up? This is rather cute.

Hoddeson:

I'll have to look more carefully.

Bloch:

That is solution, that one gets the free energy from... equation, but it's not — it sort of replaces the... of temperature.

Hoddeson:

(to visitor) This is a good time for you to show up. We're just on the last paper, the one that you read. (crosstalk) He read this paper more carefully than I did.

Man:

I was very impressed by that paper.

Bloch:

Which one? That one?

Man:

This one. There are things in this paper that have become ordinary knowledge.

Bloch:

I told your wife that in order to become a... in Germany, you have to write a schritft something, you have to display what a scholar you were, so, everything I knew came into there. You know, this idea, the connection between the partition function, that was a little joke in this paper. How your differential equation — the free energy? I'm proud

of that. You see, the idea was very simple. In a Schrodinger equation, everything depends equally, i.e., t . And in the free energy it's equal, that e minus e over $k t$, so that if you place $i e$ by i times i over k

Man:

We know the trick, yes.

Bloch:

You know that? I point it out here.

Man:

I see. That is tremendously important in the whole.

Hoddeson:

Here's the question you had about the Wigner —

Man:

The representation —

Bloch:

— let me just get this little joke here. Here,
(German)

Hoddeson:

This is page 305.

Bloch:

It had very little to do with it. See, here. It's just one of those things. You had another question?

Man:

Here, in this — representation —

Bloch:

Oh, ja, ja, ja.

Man:

Had you invented that?

Bloch:

Yes, I think I did.

Man:

I see, because that is known as Wigner's trick, I think.

Bloch:

Maybe so, I guess so. Wigner probably saw it in a much more deep connection. You see, I was well prepared to write this paper, because without publishing anything — I had been in the hospital, I was with a broken leg — the summer before — and during that time I just, in order to get not too much bored, thought about all kinds of ways of representation, also polynomials and so forth, so I had that all in the back of my mind when I wrote that paper.

Hoddeson:

On page 323 there's a — equation that is almost the Ginsberg-Landau.

Bloch:

Oh yes, yes, yes. I mean, that's another thing. He was a little angry, Landau. He never gave me any credit for that. That was the first I said, let's use the ψ as a continuous function. I was really annoyed about Landau, because, the first time I told about it was on my visit to Russia, and there — that was in

Kharkov and Tom was there, Landau was there, and I told them —

Hoddeson:

This was in '34?

Bloch:

'31. '31. And Landau was there. Well, he published that, so — that's true. I mean, this is the first time, parameter angles.

Hoddeson:

You certainly talked to Landau, because on page 321 you have a long footnote about Landau pointing, you must have been in contact with Landau in this period.

Bloch:

Yes. Yes.

Hoddeson:

You contact — that the spin orbit coupling was more important than the dipole —

Bloch:

Yes. I don't know that, whether he told me that verbally or whether he wrote to me. I would have to think about that a little more. Yes.

Hoddeson:

Well, then —

Bloch:

— as I told you, Landau and I knew each other very well, and in many ways our thoughts were in parallel, not entirely without — I mean, there was a good deal of coupling.

Hoddeson:

You have the calculation for the wall in here and everything. (crosstalk) ... You were in Leipzig. I think we may write to you with more questions about this paper.

Bloch:

Well, I hope I can answer them. Well, actually, you know, I would almost prefer, I don't like very much to write.

Hoddeson:

That or another visit?

Bloch:

Is it that much.

Hoddeson:

I don't know.

Bloch:

If we could finish it today, I'd just as well.

Hoddeson:

OK, let me go on to some other.

Bloch:

I have another half hour. Unless you are?

Hoddeson:

I don't know, what time do you? You're not constrained either. OK, then, let me go back and see — this is the main wall — How was this paper received by other people who were interested in

ferromagnetism? Did you get a lot of feedback immediately? Do you remember any?

Bloch:

Oh, I think so.

Hoddeson:

It was really quite a — you don't remember, OK. I would like to go back to, now we're up to, we're in Leipzig in the spring of 1931.

Bloch:

In the spring of 1931, in the spring of '31? I came already in the fall of '30. I didn't do much in the winter but in the summer I got busy, yes.

Hoddeson:

Yes. Now, during this period, Wilson was there. And this was —

Bloch:

Was it that late?

Hoddeson:

Well, this is the first of his papers. It was sent in June, 1931, so he must have —

Bloch:

Ah, I was already Pauli's assistant. I didn't realize it was so late. Yes. Yes.

Hoddeson:

You were in Leipzig at this time.

Bloch:

Yes, I was Pauli's assistant.

Hoddeson:

He gave his two colloquia on semiconductors and the electron theory in general.

Bloch:

I'm sure I was there. Yes.

Hoddeson:

I was wondering if you remembered anything other than that argument you had.

Bloch:

Well, you see, I had pretty much forgotten about it, until somebody from England wrote to me and he had spoken to Wilson, and then the whole thing came back to me, and then I read my old thesis again and all of a sudden I realized — but at the time I wasn't very clear about it. That was much much later, really. I mean, I thought that Wilson had had this idea right away, but apparently it was not until '31.

Hoddeson:

No, it's not, it's quite late '31.

Bloch:

It seems so obvious now that people thought — well, anyway.

Hoddeson:

So this idea of metals as solids with unfilled bands, insulators with —

Bloch:

That was Wilson's.

Hoddeson:

Not until 1931?

Bloch:

That's right.

Hoddeson:

And the credit is really to Wilson.

Bloch:

Absolutely.

Man:

Did you discuss Bethe's thesis?

Hoddeson:

Yes, and Bloch suggested that that diagram is probably in Avard's work already.

Bloch:

Yes, Avard had in fact a book on the dynamic of theory of X-ray scattering, this same problem, what happens if a wave hits a lattice, here, and comes back, and I think, Bethe also... 's paper.

Man:

The question was, I don't know if you covered it, had you read Bethe's thesis at the time you were writing your own?

Bloch:

I really don't remember. I really don't remember. Anyhow, I don't know that either Bethe or I would have thought that the presence of a gap and reflection of X-rays were —

Man:

Because he practically invents the band gap. But he doesn't quite say it.

Bloch:

Well, that's right. That's right.

Hoddeson:

OK, then you have this article which must have been a talk in the... We already looked at this briefly. This is just in the same period.

Bloch:

Oh yes.

Hoddeson:

After the summer of —

Bloch:

Yes, yes, yes.

Hoddeson:

Yes, now, there's a reference to —

Bloch:

You showed that to me before.

Hoddeson:

Yes, I did, that's right. The subject is band theory as well as photoelectric emission and things like that. In this paper, this is actually the first really clear discussion of the bands idea that I've come across.

Bloch:

Well, I also had this sort of thing. I mean, this break —

Hoddeson:

No, here I meant the metal —

Bloch:

— oh, metal insulator —

Hoddeson:

— insulator here, in terms of these pictures that are due to —

Bloch:

— yes, well, that was just a popularization of Wilson's ideas, I would call it.

Hoddeson:

I see. OK. (crosstalk)

Bloch:

You see,... sort of more general audience, and so you sort of tried to make it simple.

Hoddeson:

OK, that's what I wanted to know, what Wilson's input was, because the only place you mention Wilson is when you discuss the semiconductor.

Bloch:

Oh, that's not decent of me. I should have ... before.

Hoddeson:

OK, that's what I wanted to know. There's no reference to Briand?

Bloch:

No. No.

Hoddeson:

Peierls of course for the weak potential case.

Bloch:

Well, I told you, I really only met Briand two years later. Personally. And already you say, his...well, when I wrote this paper, I didn't pay much attention.

Hoddeson:

That suggests that his work wasn't being circulated or talked about very much in Germany.

Bloch:

I would think so or I probably would have heard about it, yes.

Hoddeson:

OK, that's interesting.

Bloch:

Well, but you see, so often with these things, the importance of what one did often came out much later. Probably Briand didn't think either that his...was important.

Hoddeson:

Then you go to Copenhagen in 1931 via Soviet Union, and Landau's invitation.

Bloch:

Yes. Yes.

Man:

Was Landau in Copenhagen or Soviet Union then?

Bloch:

No, no, he was in Russia then. I met him before in Copenhagen, and at that time he and – and I went to Russia, saw him in Kharkov.

Hoddeson:

Did you discuss—

Bloch:

— then also in Leningrad —

Hoddeson:

— discuss ferromagnetism and superconductivity with Landau, also with Niels Bohr?

Bloch:

Ja, with Niels Bohr later, yes yes. Bohr was very much interested in superconductivity. But Landau, I don't really know. I mean I had pretty much given up on superconductivity at that time, I mean, when I was in Russia, but then Bohr had gone into this thing

and he was very much interested, had all kinds of great ideas.

Man:

What was the role of superconductivity? Was there a feeling that the theory may be wrong because you didn't?

Bloch:

No, it was, I told it to — it was one of the puzzles, that physics knew it was a very strange thing.

Man:

Yes, but the feeling was that it was solvable within the framework?

Bloch:

We tried it certainly, yes. But it was harder than before.

Hoddeson:

Are there any other strong memories of your visit to the Soviet Union? You visited Kharkov, Moscow and Leningrad?

Bloch:

Yes. I met Thom at that time and was very much impressed. In fact I stayed at his home in Moscow with him.

Hoddeson:

In what way did he influence your work?

Bloch:

Well, I don't know that he influenced my work directly, but there was again a feeling of a man who was very much, as I said, very good thinker. Thom and I became very good friends at that time.

Hoddeson:

While you were in Copenhagen I guess you wrote your review on magnetism, is that correct?

Bloch:

Yes, I think so. Yes. In fact I mentioned that in my paper here. Peierls (crosstalk) reference to that. That's really amusing. Here.

Hoddeson:

This is the letter —

Bloch:

— while I waited for Bohr, I, like, you know what Born says ? Born says one of his priests, his high priests, like the old high priests, and I say, I don't mean to hit Pauli by that, I'm writing my handbook article and at the moment I'm annoyed that there is something so stupid as thermodynamics. Amusing. I also say, "Bohr hasn't come yet. I hope that God will give me long enough life that I will still see him some day." Amusing. It is so typical of the way we wrote at that time. The jokes of physics were not separated from each other.

Hoddeson:

Was that your main work in Copenhagen, writing the review on —

Bloch:

— oh no, no, no. My main work was on the stopping [?] power of particles in matter.

Hoddeson:

And did you discuss that with Bohr?

Bloch:

Oh yes. Have you seen my reminiscences of Niels Bohr?

Hoddeson:

Yes.

Bloch:

You have that. I mentioned that there, at that time. I knew nothing about the problem. It was an old problem that Bohr had done in 1930. Then I picked it. That was my main work in Copenhagen, and Bohr was very much interested in that. I mean, superconductivity came out only marginally, I would say.

Hoddeson:

I see. OK. But then you went back to superconductivity, didn't you, in about 1932 or so?

Bloch:

Not really. Not really. I came back to it very much later, in connection with the effect, but that was over ten years.

Hoddeson:

Were you still in Leipzig, at the 1933 conference that was held there on ferromagnetism?

Bloch:

I don't think so. That was already after Hitler was in. No, I didn't go.

Hoddeson:

You left —

Bloch:

— in the spring of '33.

Hoddeson:

In the spring of '33, not because you were thrown out, but because you thought that, the writing was on the wall.

Bloch:

Of course, and then, with my Jewish name — besides, I — after all, I despised those guys from the beginning. But you said, I saw the writing on the wall. This is true. In... before Hitler came to power,

already I applied for a Rockefeller fellowship. So, when he came to power, he came to power a little earlier than I thought, but I had already my Rockefeller Fellowship, so there was only the summer between — and that's when I went to Paris. And to Copenhagen.

Hoddeson:

You went home to Switzerland for a while.

Bloch:

Yes.

Hoddeson:

Not for very long though?

Bloch:

Well, yes, I spent more or less the whole summer there. Except for Paris in between. And Italy.

Hoddeson:

You told Weiner that you got an ironic letter from the dean of the university asking you to come back and teach your courses, and that they would provide guards. Incredible story.

Bloch:

Incredible, yes.

Hoddeson:

Then you went to Paris, where you taught briefly.

Bloch:

Yes.

Hoddeson:

You mentioned that already.

Bloch:

Well, I gave some lectures at the — yes. I stayed at the —

Hoddeson:

Right, and you mentioned that you met Briand there.

Bloch:

Right.

Hoddeson:

Did you see him very frequently?

Bloch:

Well, I don't remember.

Hoddeson:

But you did talk about physics? and everything else.

Bloch:

Oh sure, of course. Look, that was again a very close knit group. There was... and Briand and well, all these people there,... We worked together all the time.

Man:

Did you ask the Peierls question?

Hoddeson:

I brought it up.

Bloch:

Ja, somehow, I don't quite know why it is that Briand — I said to your wife, it may have been that I spoke French. My communication with Briand was simply better than Peierls'. Peierls I don't think spoke French.

Hoddeson:

In Italy in 1933, you say in your interview with Weiner that you discussed second quantization, with Fermi, in connection with his theory of the neutrino. Had you any ideas of applying some of this field theory to solid state physics might help with some of the problems?

Bloch:

Well, not solid state physics so much. You see, I used second quantization in a different way, because I had before written a paper on stopping power, where I considered the atom as a gas, you know, and there I felt that again, it was a sort of idea, that could no longer use as a parameter. I know, it was, and then, I hit on the second quantization. I told Fermi about it, and I think I told that too — Fermi said, “I don’t understand a word,” and then Fermi... I did think of that application. No, but you see, I wrote, if I may say so, I wrote a paper when I was in Rome on the propagation of sound waves in a Fermi gas. I mean, people say nowadays, how I created bozons out of fermions. Yes, well... I can give it to you, the reference, if you want it.

Hoddeson:

Yes, I want it. Because you mention in the Kuhn interview that while you were in Rome, you wanted to understand how plasma oscillations, and the oscillations of an electron gas could be explained in terms of the quantized amplitudes.

Bloch:

Ja, ja.

Hoddeson:

I didn't realize that plasma oscillations were in quantum systems, where were thought of before the fifties?

Bloch:

Here, this is it. (German)

Hoddeson:

That's 1934, OK. 1934, this is.

Bloch:

Maybe the next volume. Oh, here it comes, I think. Yes, that's it. Here we are.

Hoddeson:

Oh, there, it's in... I can just dictate the reference, then I can get it. OK. So this is incoherent —

Bloch:

— incoherent scattering of X-rays and propensity for fluctuations in a degenerate Fermi gas.

Hoddeson:

Right, OK, and the volume here is — volume 7, OK, 1934. And the page number is 385. OK, I will get that.

Bloch:

That was the sort of, in that context, OK?... came up the propagation of fields and the, I discussed it with Fermi and Fermi didn't understand a word, up until this paper on neutrinos. That was the great wonderful thing of Fermi. Not that he was in that sense, but that if somebody was not completely clear he'd say, "Stop." Then I say, "Thank God, I have to think about it." If even Fermi cannot understand it.

Man:

This is entirely the charged Fermi gas?

Bloch:

Well, I thought of electrons, yes. I mean, the charged growing out of — again, growing out of going to a paper of Heisenberg, Heisenberg had a paper on... but I looked at it in an additional way. It's the same thing as the scattering of light in the sky. There's the radio approach, where you consider scattering of individual electrons and fluctuations, and then there's Einstein's approach, where you talk about the scattering of light on sound waves, and mine is sort of an application of that to the fluctuation of a Fermi gas.

Hoddeson:

Did you use the words plasma oscillations in that period?

Bloch:

No.

Hoddeson:

That was invented later.

Bloch:

Yes, I'm sure. Again, it didn't surprise me very much I just followed a Fermi gas as such, and I said, well, after all, there ought to be sound waves in it, how are they excited?

Hoddeson:

Right. Very interesting. Once you got to Stanford in 1933, your interest shifted to non-solid state subjects.

Bloch:

Yes, because people here worked on other things. I did some X-ray work.

Hoddeson:

Right. You did give a seminar, you told Weiner, on Fritz London's theory of superconductivity in '34 or '35.

Bloch:

Yes, that was a joint seminar we had with Oppenheimer, yes.

Hoddeson:

Yes. You must have been still following it closely.

Bloch:

Yes, sure, sure.

Hoddeson:

Had you heard about the Meitner effect? at that time?

Bloch:

Yes. Yes, I think so, because that's what —
(crosstalk)

Hoddeson:

— London theory was all about, of course.

Bloch:

Of course.

Hoddeson:

OK, what I would like to do now, just in the last five or ten minutes, is generalize our discussion just a little bit, and talk about the period 1926-33. As historians, we're all obsessed with delineating and characterizing historical periods.

Bloch:

Why is '33 for you a — ?

Hoddeson:

Well, it seems to me that, at that period, first of all, there was the emigration, and many people switched from solid state at that time who were working in it, including you and Peierls and Bethe. And while other people entered who hadn't been working in solid state at that time, people like Mott and Wigner.

Bloch:

That's true, yes.

Hoddeson:

So there was a change of people.

Bloch:

Of personnel, yes.

Hoddeson:

A change of personnel. There was also a change in approach, it seems to me, the Wigner-Seitz paper came out in '33, and that seems to mark the beginning of more quantitative calculations, focused on particular —

Bloch:

— yes, quite, quite...

Hoddeson:

— solids, where before they were general.

Bloch:

Right. That is very true.

Hoddeson:

Also at that time there was a surge of review articles, not only yours, but Sommerfeld-Bethe, I mean, '32, '34, there was a big surge —

Bloch:

— I realize, it was sort of the end of a certain period
—

Hoddeson:

— summing up the time. And also it was a time when — well, this was the time when the textbooks were beginning, the subject was being summarized, so that's why I call it a breaking point, to a degree.

Bloch:

Well, now what you say, I see. I didn't quite understand before, but now you explain it, yes. It's true, yes.

Hoddeson:

And that period seems to begin about 1926 with the Pauli work.

Bloch:

Yes, I think so. Pauli was a start, yes.

Hoddeson:

So that seems to be a period. But there seems to be a break. It's not just '26 to '33. At about '28, there seems to be a change also. Up till '28 it seems to be a kind of developing the tools, whereas from '28 to '33, it looks to me as though it were a time when things were being worked out.

Bloch:

Yes.

Hoddeson:

Do you agree with that?

Bloch:

Yes, I think that's true.

Hoddeson:

OK — did the political events in '33 have a direct impact on your changing fields?

Bloch:

Of course. Oh yes, definitely, yes.

Hoddeson:

You probably would have stayed in solid state.

Bloch:

More by the fact that we were put in a new environment. For example, my paper on the theory of different times came because I spent a summer in Zurich, as you remember, and there, I think Pauli told us about Dirac's theory where he introduces different times, poly [?] for each particle. I wrote a paper because I was there already by then.

Hoddeson:

Right. Partly it was that they were new and exciting discoveries in quantum —

Bloch:

I would say it was more the new environment to which we were, you might say, forced. Hitler did me a great deal of favor, but he didn't intend it.

Hoddeson:

In retrospect, you were writing two big reviews at that time, magnetism and (crosstalk)

Bloch:

Yes...

Hoddeson:

And in this period, did it feel as though this were the end and the beginning of an era? While you were writing those?

Bloch:

Well, I don't know that I thought in these terms one way or the other. I mean, I was getting paid for writing up articles. So I wrote articles.

Hoddeson:

Lots of people were being asked at that time to write articles. Do you remember?

Bloch:

Ja. Well, I think that was more the job of the editors. They were the smart people who said "This is time."

Hoddeson:

"This is the end of an era."

Bloch:

And we — being very mercenary people they offered money for our articles and we wrote articles.

Hoddeson:

Who was the editor at that time of the —

Bloch:

(crosstalk)

Hoddeson:

I see.

Bloch:

The HANDBOOK OF RADIOLOGY.

Hoddeson:

At that time, I see.

Bloch:

That was Marx, I think. Not the founder of Socialism.

Hoddeson:

No. Well, we've run out of my questions.

Bloch:

Well, fine, I think that timing was very good. Yes, I mean, if you have more questions, but I would rather, as I say, I find it very difficult in writing because I try to be too complete and so on.

Hoddeson:

May I call you on the telephone with questions?

Bloch:

Yes. You can also write to me. If my answer doesn't come soon or so, I wish, I didn't have to write too much. I'm just lazy.

Hoddeson:

We're the same way.

Bloch:

Just a lazy man, that's all.

Man:

Another alternative is what we did with Peierls, to ask questions on a cassette tape. She sent to Peierls questions on a cassette tape and he just dictated answers back on the tape.

Bloch:

Oh, ja. Well, that — I don't particularly like.

Hoddeson:

OK. Each person has his or her own way of dealing with things.

Bloch:

Well, Peierls is not as lazy as I am.

Hoddeson:

Well, thank you very very much.

